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ADDRESS OF THE PRESIDENT TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

IT is with much misgiving that I endeavor to discharge the traditional duty of the president of a section of the British Association. So many other duties seem to find a natural resting-place with any one who has to reckon at the same time with the immediate requirements of the public, the claims of scientific opinion, and the interests of posterity, that, unless you are content with such contribution towards the advancement of the sciences of mathematics and physics as my daily experience enables me to offer you, I shall find the task impossible.

With a leaning towards periodicity perhaps slightly unorthodox I have looked back to see what they were doing in Section A fifty years ago. Richard Owen was president of the association, William Whewell was president of Section A for the fifth time.

At the meeting of 1858 they must have spent some time over nineteen very substantial reports on researches in science, which included a large section of Mallett's facts and theory of earthquake phenomena, magnetic surveys of Great Britain and of Ireland, and, oddly enough, an account of the self-recording anemometer by Beckley; perhaps a longer time was required for fifty-seven papers contributed to the section, but very little was spent

[1] Dublin, 1908.

over the presidential address, for it only occupies two pages of print. My inclination towards periodicities and another consideration leads me to regard the precedent as a good one. That other consideration is that Section A has always more subjects for discussion than it can properly dispose of; and, in this case, discipline, like charity, might begin at home.

Since the section met last year it has lost its most illustrious member and its most faithful friend. Lord Kelvin made his first contribution to Section A at Cambridge in 1845, on the elementary laws of statical electricity; he was president of the section in 1852 at Belfast for the first of five times. I have looked to see what suggestion I could derive from his first essay in that capacity. I can find no reference to any address in the published volume. I wish I had the courage to follow that great example.

Lord Kelvin's association with Section A was so constant and so intimate that it requires more than a passing word of reference. There is probably no student of mathematics or physics grown into a position of responsibility in this country but keeps among his treasured reminiscences some words of inspiration and of encouragement from Kelvin, spoken in the surroundings which we are once more met to inaugurate. I refer to those unrecorded acts of kindness and help because they were really a striking characteristic of Section A. Their value for the amenity as well as for the advancement of science it would be difficult to over-estimate. I could not, even if time permitted, hope to set before you an adequate appreciation of Kelvin's contributions to science as illustrated by his communications to this section, and in this place it is not necessary. But I can not pass over that feature of his character without notice.

Closely following on the loss of Kelvin came the death of Sir Richard Strachey, a personal loss to which it is difficult to give expression. I am not aware that he had much to do with Section A. I wish, indeed, that the section had seen its way to bring him more closely into touch with its proceedings. He was president of Section E in 1875, and, by appointment of the Royal Society, he was for twenty-two years chairman of the meteorological council. I had the good fortune to be very closely associated with him during the last ten years of his life, and to realize the ideas which lay behind his official actions and to appreciate the reality of his services to science in the past and for the future.

These losses unfortunately do not stand alone. Only last year Sir John Eliot received the congratulations of all his fellow-workers upon the publication of his "Climatological Atlas of India" as representing the most conspicuous achievement of orderly, deliberate, purposeful compilation of meteorological facts for a special area that has yet been seen. He was full of projects for a handbook to accompany the atlas, and of ideas for the prosecution of meteorological research over wide areas by collecting information from all the world and enlisting the active cooperation of the constituent parts of the British empire in using those observations for the advancement of science and the benefit of mankind. He died quite suddenly on March 18, not young as years go, but quite youthful in the deliberate purpose of manifold scientific activities and in his irrepressible faith in the future of the science which he has adorned.

The section will, I hope, forgive me if I put before them some considerations which the careers of these three men suggest. Kelvin, a mathematician, a natural philosopher, a university professor, some part of whose scientific work is known to each one of us. He was possessed with the notion that

mathematics and natural philosophy are applicable in every part of the work of daily life, and made good the contention by presenting to the world, besides innumerable theoretical papers, instruments of all degrees of complexity, from the harmonic analyzer to an improved water-tap. It was he who transfigured and transformed the mariner's compass and the lead-line into instruments which have been of the greatest practical service. It was he who, when experimental science was merely a collection of facts or generalizations, conceived the idea of transfiguring every branch of it by the application of the principles of natural philosophy, as Newton had transfigured astronomy. The ambition of Thomson and Tait's "Natural Philosophy," of which only the first volume reached the stage of publication, is a fair index of Kelvin's genius.

Strachey, on the other hand, by profession a military engineer, a great administrator, head of the Public Works Department in India, deeply versed in finance and in all the other constituent parts of administration, by his own natural instinct demanded the assistance of science for every branch of administration. In promoting the development of botany, of meteorology, or geodesy and of mathematics, he was not administering the patronage of a Macænas, but claiming the practical service of science in forestry, in agriculture, in famine relief, in public works, and in finance. You can not gauge Strachey's services to science by the papers which he contributed to scientific societies, if you leave out of account the fact that they were really incidents in the opening of fresh channels of communication between scientific work and the public service.

And Eliot, as meteorological reporter to the government of India, an accomplished mathematician (for he was second wrangler and first Smith's prizeman in 1869), a capable and devoted public servant, the medium by which Strachey's ideas as regards the use of meteorology in administration found expression in the government of India, who caught the true perception of the place of science in the service of the state, and made his office the indispensable handmaid of the Indian administration. These three men together, who have all passed away within a space of three months, are such representative types of scientific workers, complementary and supplementary, that a similar combination is not likely to occur again. All three indispensable, yet not two alike, except in their enthusiasm for the sciences for the advancement of which Section A exists.

To these I might indeed add another type, the private contributor to the physical exploration of the visible universe, of which Ireland furnishes so many noble examples; and in that connection let me give expression to the sense of grievous loss, to this association and to science, occasioned by the premature death of W. E. Wilson, of Daramona, a splendid example of that type.

In the division of the work of advancing the science of mathematics and physics and their application to the service of mankind, I am reminded of Dryden's somewhat lopsided comparison of the relative influence of music and song in his Ode to St. Cecilia's Day. If I may be pardoned for comparing small things with great, the power of Timotheus's music over Alexander's moods was hardly less complete than Kelvin's power to touch every department of the working world with his genius. But I may remind you that, after a prolonged description of the tremendous influence of Timotheus upon the victorious hero, the poet deals in one stanza with his nominal subject:

> At last divine Cecilia came,
> Inventress of the vocal frame;
> The sweet enthusiast, from her sacred store,
> Enlarged the former narrow bounds
> . . . . . .
> With nature's mother-wit, and arts unknown before.
>
> Let old Timotheus yield the prize,
> Or both divide the crown;
> He raised a mortal to the skies,
> She drew an angel down.

I doubt if any of my hearers who knew Strachey by sight would recognize in him the scientific reincarnation of St. Cecilia, but it is none the less true that he was pre-eminent among men in inventing the means of drawing angels down and using their service for the attuning of common life to a scientific standard. It may be equally hard for those who knew him to look upon Eliot as a vocal frame, for of all his physical capacities his voice was the least impressive; and yet it is not untrue to say that he was conspicuously a medium by which the celestial harmonies of the physical sciences were brought into touch with the practical life of India through his work, which is represented by a considerable number of the twenty volumes of *Memoirs* of the Indian Meteorological Service.

I do not indulge in this poetic extravagance without some underlying reason. Speaking for the physics of the atmosphere, there is a real distinction between these three sides of scientific work. To some is given the power of the mathematician or the physicist to raise the mortal to the skies, to solve some problem which, if not in itself a meteorological one, still has a bearing, sooner or later to be discovered and developed, upon the working of atmospheric phenomena. It is easy enough to cite illustrious examples: among notable instances there recur to my mind Rayleigh's work on the color of the sky and Pernter's meteorological optics; papers by Ferrel and others on the general circulation of the atmosphere; Kelvin and Rayleigh on the elastic oscillations of the atmosphere; the papers by Hagen, Helmholtz, Oberbeck, Margules, Hertz and Von Bezold on the dynamics and thermodynamics of the atmosphere, collected and translated by Cleveland Abbe; the work on atmospheric absorption by Langley and the theoretical papers on radiation by Poynting; those on condensation nuclei by Aitken and Wilson, and the recent work on atmospheric electricity, including the remarkable paper by Wilson on the quiet transference of electricity from the air to the ground.

But these things are not of themselves applied to the meteorology of every-day life. It is, in a way, a separate sense, given to few, to realize the possibilities that may result from the solution of new theoretical problems, from the invention of new methods—to grasp, in fact, the idea of bringing the angels down. And, in order that the regular workers in such matters may be in a position constantly to reap the advantages which men of genius provide, the vocal frame must have its permanent embodiment. For the advancement of science in this sense we require all three—the professor with academic freedom to illuminate with his genius any phenomenon which he may be pleased to investigate, the administrator, face to face with the practical problems in which science can help, and the living voice which can tune itself in harmony with the advances of science and in sympathy with the needs of the people whom it serves.

The true relations of these matters are not always apparent. Eliot, bringing to the work of the Indian Meteorological Office a mind trained in the mathematical school of which Kelvin was a most conspicuous exponent, achieved a remarkable

success, with which perhaps my hearers are not familiar.

In this country there is a widespread idea that meteorology achieves its object if by its means the daily papers can give such trustworthy advice as will enable a cautious man to decide whether to take out his walking-stick or his umbrella. Some of us are accustomed to look upon India as a place of unusual scientific enlightenment, where governments have a worthy appreciation of the claims of science for recognition and support. But Eliot was never tired of telling me that it was the administration of India, and not the advancement of science, that the Indian administrators had in view; and among his achievements the one of which he was most proud was that the conduct of his office upon scientific lines during his tenure had so commended itself to the administrators that his successor was to be allowed three assistants, with special scientific training, in order that the state might have the benefit of their knowledge.

It is, of course, easy to suggest in explanation of this success that the Department of Public Works of India can not afford to be unmindful of the distribution of rainfall, and that there is an obvious connection between Indian finances and Indian droughts; but it is a new fact in British history that the application of scientific considerations to the phenomena of rainfall are of such direct practical importance that meteorological information is a matter of consequence to all government officials, and that meteorological prospects are a factor of finance. Imagine his Majesty's Chancellor of the Exchequer calling at 63 Victoria Street to make inquiries with a view to framing his next budget, or taking his prospects of a realized surplus from the "Daily Weather Report." Yet in India meteorology is to such an extent a public servant that such proceedings would not excite remark.

To have placed a scientific service on such a footing is, indeed, a notable success. Again, I rely upon Eliot when I say that that success is only to be achieved by being constantly on the watch to render service wherever service can be rendered. There is a difference between this attitude and that which has for its object the contribution of an effective paper to a scientific publication; in other words, it must be frankly recognized that the business of the scientific departments of government is not to raise an occasional mortal to the skies, but to draw down as many angels as are within reach. I was much surprised, when Eliot wished to develop a large scheme for meteorological work on a wider scale, that he made his appeal to the British Association as chairman of the Subsection for Cosmical Physics at Cambridge, and thereby to the governments of this country and the colonies. He felt that he could only urge the Indian government to join, and he did so successfully, so far as India would be directly benefited thereby, however important the results might be from a purely scientific point of view. Strange as it may appear to some, it was to this country that he looked for assistance, on the plea of the increase of knowledge for its own sake, or for the sake of mankind at large.

I am disposed, therefore, to carry your thoughts a little further, and rely on your patience while I consider another aspect for the process of drawing down the angels from the mathematical and physical sky, a process which is sufficiently indicative of the functions of a state scientific department. Viewing the world at large, and not merely that part of it with which we are ourselves immediately concerned, such departments deal with celestial physics in astronomy, with the physics of the air in meteorology and atmospheric electricity,

with the physics of land and water in physical geography and geology, seismology and terrestrial magnetism, oceanography and hydrography. It is for the practical applications of these sciences to the service of the navigator, the fisherman, the husbandman, the miner, the medical man, the engineer and the general public that there is an obvious public want.

Let me carry you with me in regarding these departments, primarily, as centers for establishing the growth of science by bringing it to bear upon the practical business of life, by a process of regular plantation, and not the occasional importation of an exotic scientific expert. I shall carry you with me also if I say that the gravest danger to such scientific institutions is the tendency to waste. I use the term "waste" not in its narrowest, but in its most liberal sense, to include waste of money, waste of effort, waste of scientific opportunity. I do not regard it as a waste that such a department should be unable to emulate Timotheus's efforts. Any aspiration in that direction is, of course, worthy of every encouragement, but the environment is not generally suitable for such achievements. I do, however, regard it as waste if the divine Cecilia is not properly honored, and if advantage is not taken of the fullest and freest use of the newest and best scientific methods, and their application in the widest manner possible.

I speak for the office with which I am connected when I say its temptations to waste are very numerous and very serious. It is wasteful to collect observations which will never be used; it is equally wasteful to decline to collect observations which in the future may prove to be of vital importance. It is wasteful to discuss observations that are made with inadequate appliances; it is equally wasteful to allow observations to accumulate in useless heaps because you are not sure that the instruments are good enough. It is wasteful to use antiquated methods of computation or discussion; it is equally wasteful to use all the time in making trial of new methods. It is wasteful to make use of researches if they are inaccurate; it is equally wasteful to neglect the results of researches because you have not made up your mind whether they are accurate or not. It is wasteful to work with an inadequate system in such matters as synoptic meteorology; it is equally wasteful to lose heart because you can not get all the facilities which you feel the occasion demands.

It is the business of those responsible for the administration of such an office to keep a nice balance of adjustment between the different sides of activity, so that in the long run the waste is reduced to a minimum. There must in any case be a good deal of routine work which is drudgery; and if one is to look at all beyond the public requirements and public appreciation of the immediate present, there must be a certain amount of enterprise and consequently a certain amount of speculation.

Let me remark by the way that there is a tendency among some of my meteorological friends to consider that a meteorological establishment can be regarded as alive, and even in good health, if it keeps up its regular output of observations in proper order and up to date, and that initiative in discussing the observations is exclusively the duty of a central office. That is a view that I should like to see changed. I do not wish to sacrifice my own privilege of initiative in meteorological speculation, but I have no wish for a monopoly. To me, I confess, the speculation which may be dignified by the name of meteorological research is the part of the office work which makes the drudgery of routine tolerable. For my part, I should like every worker in the office, no matter how humble his position may be, somehow or other to have the opportunity

of realizing that he is taking part in the unravelling of the mysteries of the weather; and I do not think that any establishment, or section of an establishment, that depends upon science can be regarded as really alive unless it feels itself in active touch with that speculation which results in the advancement of knowledge. I do not hesitate to apply to other meteorological establishments, and indeed to all scientific institutions that claim an interest in meteorology, the same criterion of life that I apply to my own office. It is contained in the answer to the question, How do you show your interest in the advancement of our knowledge of the atmosphere? The reply that such and such volumes of data and mean values measure the contribution to the stock of knowledge leaves me rather cold and unimpressed.

But to return to the endeavor after the delicate adjustment between speculation and routine, which will reduce the waste of such an institution to a minimum; experience very soon teaches certain rules.

I have said elsewhere that the peculiarity of meteorological work is that an investigator is always dependent upon other people's observations; his own are only applicable in so far as they are compared with those of others. Up to the present time, I have never known any one take up an investigation that involved a reference to accumulated data, without his being hampered and harassed by uncertainties that might have been resolved if they had been taken in time. I shall give you an example presently, but, in the meantime, experience of that kind is so universal that it has now become with us a primary rule that any data collected shall forthwith be critically examined and so far dealt with as to make sure that they are available for scientific purposes—that is, for the purposes of comparison. A second rule is that as public evidence of the completion of this most important task there shall be at least a line of summary in a published report, or a point on a published map, as a primary representation of the results. Such publication is not to be regarded as the ultimate application of the observations, but it is evidence that the observations are there, and are ready for use.

You will find, if you inquire, that at the office we have been gradually lining up these troops of meteorological data into due order, with all their buttons on, until, from the commencement of this year, any one who wishes to do so can hold a general review of the whole meteorological army, in printed order—first order stations, second order stations, rainfall stations, sunshine and wind stations, sea temperatures and other marine observations—on his own study table, within six months of the date of the observations, upon paying to his Majesty's Stationery Office the modest sum of four shillings and sixpence. For all the publications except one the interval between observation and publication is only six weeks, and as that one has overtaken four years of arrears within the last four years, I trust that by the end of this year six weeks will be the full measure of the interval between observation and publication in all departments. This satisfactory state of affairs you owe to the indefatigable care and skill of Captain Hepworth, Mr. Lempfert and Mr. R. H. Curtis, and the members of the staff of the office who work under their superintendence. I need say little about corresponding work in connection with the Daily Weather Report, in which Mr. Brodie is my chief assistant, although it has received and is receiving a great deal of attention. The promptitude with which the daily work is dealt with hardly needs remark from me, though I know the difficulties of it as well as any one. If I spend only one long sentence in mentioning that on July 1, 1908, the morning hour of ob-

servation at twenty-seven out of the full number of twenty-nine stations in the British Isles was changed from 8 A.M. to 7 A.M., and the corresponding postoffices, as well as the Meteorological Office, opened at 7:15 A.M. in order to deal with them, so that we may have a strictly synchronous international system for western and central Europe, and thus realize the aspiration of many years, you will not misunderstand me to mean that I estimate the task as an easy one.

The third general rule is that the effectiveness of the data of all kinds, thus collected and ordered, should be tested by the prosecution of some inquiry which makes use of them in summary or in detail. It is here that the stimulating force of speculative inquiry comes in; and it is in the selection and prosecution of these inquiries, which test not only the adequacy and effectiveness of the data collected but also the efficiency of the office as contributing to the advance of knowledge, that the most serious responsibility falls upon the administrators of parliamentary funds.

Scientific Shylocks are not the least exacting of the tribe, and there have been times when I have thought I caught the rumination:

Shy. Three thousand ducats? 'tis a good round sum!

Bas. For the which, as I told you, Antonio shall be bound.

. . . . . .

Shy. Antonio is a good man?

Bas. Have you heard any imputation to the contrary?

Shy. Oh! no, no, no, no. . . . Yet his means are in supposition: he hath an argosy bound to Tripolis, another to the Indies; I understand, moreover, upon the Rialto, that he hath a third in Mexico, a fourth for England, and other ventures he hath squandered abroad. But ships are but boards, sailors but men. There is the peril of water, winds and rocks. . . . Three thousand ducats.

We at the Meteorological Office are very much in Antonio's position. Our means of research are very much in supposition: four observatories and over four hundred stations of one sort or another in the British Isles; an elaborate installation of wind-measuring apparatus at Holyhead; besides other ventures squandered abroad; an anemometer at Gibraltar, another at St. Helena; a sunshine recorder at the Falkland Isles, half a dozen sets of instruments in British New Guinea, and a couple of hundred on the wide sea. The efforts seem so disconnected that the rumination about the ducats is not unnatural.

And you must remember that we lack an inestimable advantage that belongs to a physical laboratory or a school of mathematics, where the question of the equivalent number of ducats does not arise in quite the same way. The relative disadvantage that I speak of is that in an office the allowance for the use of time and material in practise and training disappears. All the world seems to agree that time or money spent on teaching or learning is well spent. In the course of twenty years' experience at a physical laboratory, and in examinations not a few, I have seen **M** and **N** or the wave-length of sodium light determined in ways that would earn very few ducats on the principle of payment by results; but, having regard to the psychological effect upon the culprit or the examiner, the question of ducats never came in. Wisely or unwisely public opinion has been educated to regard the psychological effect as of infinite value compared with the immediate result obtained. But in an office the marks that an observer or computer gets for showing that he "knew how to do it," when he did not succeed in doing it, do not count towards a "first class," and we have to abide by what we do; we can not rely on what we might have done. Consequently our means in supposition, spread over sea

and land, are matters of real solicitude. In such circumstances there might be reason for despondency if one were dependent merely upon one's own ventures and the results achieved thereby. But when one has the advantage of the gradual development of investigations of long standing, it is possible to maintain a show of cheerfulness. When Shylock demands his pound of flesh in the form of an annual report, it is not at all uncommon to find that some argosy that started on its voyage long ago "hath richly come to harbor suddenly." There have been quite a number of such happy arrivals within the last few years.

I will refer quite briefly to the interesting relations between the yield of barley and cool summers, or the yield of wheat and dry autumns, and the antecedent yield of eleven years before, which fell out of the body of statistics collected in the "Weekly Weather Report" since 1878. The accomplished statisticians of the Board of Agriculture have made this work the starting-point for a general investigation of the relation between the weather and the crops, which can not fail to have important practical bearings.

Let me take another example. For more than a full generation meteorological work has been hampered by the want of a definite understanding as to the real meaning in velocity, or force, of the various points of the scale of wind estimates laid down in 1805 by Admiral Beaufort for use at sea, and still handed on as an oral tradition. The prolonged inquiry, which goes back really to the report upon the Beckley anemograph already referred to, issued quite unexpectedly in the simple result that the curve

$$p = .0105B^3$$

(where p is the force in pounds per square foot, and B the arbitrary Beaufort number) runs practically through nine out of the eleven points on a diagram representing the empirical results of a very elaborate investigation. The empirical determinations upon which it is based are certainly not of the highest order of accuracy; they rely upon two separate investigations besides the statistical comparison, viz., the constant of an anemometer and the relation of wind velocity to wind pressure, but no subsequent adjustment of these determinations is at all likely to be outside the limits of an error of an estimate of wind force; and the equation can be used, quite reasonably, as a substitute for the original specification of the Beaufort scale, a specification that has vanished with the passing of ships of the type by which it was defined. This result, combined with the equation $p = .003V^2$, which has been in use in the office for many years, and has recently been confirmed as sufficiently accurate for all practical purposes by Dr. Stanton at the National Physical Laboratory and Monsieur Eiffel at the Eiffel Tower, places us upon a new plane with regard to the whole subject of wind measurement and wind estimation.

Results equally remarkable appear in other lines of investigation. Let me take the relation of observed wind velocity to barometric gradient. You may be aware that in actual experience the observed direction of the wind is more or less along the isobars, with the low pressure on the left of the moving air in the northern hemisphere; and that crowded isobars mean strong winds. Investigations upon this matter go back to the earliest days of the office.

There can be no doubt that the relation, vague as it sometimes appears to be upon a weather chart, is attributable to the effect of the earth's rotation. In order to bring the observed wind velocity into numerical relation with the pressure gradient Guldberg and Mohn assumed a coefficient of surface "friction," interfering with the steady

motion. The introduction of this new quantity, not otherwise determinable, left us in doubt as to how far the relation between wind and pressure distribution, deducible from the assumption of steady motion, could be regarded as a really effective hypothesis for meteorological purposes.

Recent investigations in the office of the kinematics of the air in traveling storms, carried out with Mr. Lempfert's assistance, have shown that, so far as one can speak of the velocity of wind at all—that is to say, disregarding the transient variations of velocity of short period and dealing with the average hourly velocity, the velocity of the wind in all ordinary circumstances is effectively steady in regard to the accelerating forces to which it is subject. This view is supported by two conclusions which Mr. Gold has formulated in the course of considering the observations of wind velocity in the upper air, obtained in recent investigations with kites. The first conclusion is that the actual velocity of wind in the upper air agrees with the velocity calculated from the pressure distribution to a degree of accuracy which is remarkable, considering the uncertainties of both measurements; and the second conclusion affords a simple, and I believe practically new, explanation upon a dynamical basis of the marked difference between the observed winds in the central portions of cyclones and anti-cyclones respectively, by showing that, on the hypothesis of steady motion, the difference of sign of the effective acceleration, due to curvature of path and to the earth's rotation respectively, leads to quite a small velocity and small gradient as the limiting values of those quantities near anti-cyclonic centers.

This conclusion is so obviously borne out by the facts that we are now practically in a position to go forward with the considerable simplification which results from regarding the steady state of motion in which pressure gradient is balanced by the effective acceleration due to the rotation of the earth and the curvature of the path, as the normal or ordinary state of the atmosphere.

I can not forbear to add one more instance of an argosy which has richly come to harbor so lately as this summer. You may be aware that Kelvin was of opinion that the method of harmonic analysis was likely to prove a very powerful engine for dealing with the complexities of meteorological phenomena, as it has, in fact, dealt with those of tides. In this view Sir Richard Strachey and the Meteorological Council concurred, and an harmonic analyzer was installed in the office in 1879, but subsequently numerical calculation was used instead. A considerable amount of labor has been spent over the computation of Fourier coefficients. Not many great generalizations have flowed from this method up to the present time. I have no doubt that there is much to be done in the way of classifying temperature conditions, for climatic purposes, by the analysis of the seasonal variations. A beginning was made in a paper which was brought to the notice of the association at Glasgow. The most striking result of the Fourier analysis we owe to Hann, who has shown that, if we confine our attention to the second Fourier coefficient of the diurnal variation of pressure—that is, to the component of twelve-hour period—we get a variation very marked in inter-tropical regions, and gradually diminishing poleward in both hemispheres, but synchronous in phase throughout the 360 degrees of a meridian. The maximum occurs along all meridians in turn about 10 A.M. and 10 P.M. local time. This semi-diurnal variation with its regular recurrence is well known to mariners, and we have recently detected it, true to its proper phase, in the observations at the winter quarters of the *Discovery;* small in amplitude indeed—about a thousandth of

an inch of mercury—but certainly identifiable.

The reality of this variation of pressure, common to the whole earth, can not be doubted, and, so far as it goes, we may represent it (if indeed we may represent pressure differences as differences in vertical heights of atmosphere) as the deformation of a spherical atmosphere into an ellipsoid, with its longest axis in the equator pointing permanently 30° to the west of the sun. Its shortest axis would also be in the equator, and its middle axis would be along the polar axis of the earth. Somehow or other this protuberance remains fixed in direction with regard to the sun, while the solid earth revolves beneath it. Whatever may be the cause of this effect, obviously cosmical, and attributable to the sun, at which it indirectly points, its existence has long been recognized, and further investigation only confirms the generalization. It is now accepted as one of the fundamental general facts of meteorology.

Professor Schuster, for whose absence from this meeting I may venture to express a regret which will be unanimous, has already contributed a paper to the Royal Society pointing out the possible relations between the diurnal variations of pressure and those of terrestrial magnetic force. Going back again to the ubiquity of the application of the relation of pressure and wind, in accordance with the dynamical explanation of Buys Ballot's law, we should expect the effect of a pressure variation that has its counterpart in that of terrestrial magnetism to be traceable also in wind observation.

Mr. J. S. Dines has just given me particulars of the discovery of that effect in the great air-current, the variations of which I have called the pulse of the atmospheric circulation—I mean the southeast trade wind, the most persistent atmospheric current in the world. It is difficult as a rule to get observers to pay much attention to that current, because it is so steady; but in 1891 the Meteorological Council set up an anemometer at St. Helena, in the very heart of the current, and we have just got out the results of the hourly tabulations. When the observations for the hours 1 to 24 are grouped separately for months, so as to give the vector resultants for each hour and for each month, it appears that there is a conspicuous semi-diurnal variation in the current, which shows itself as a closed polygon of vector variations from the mean of the day.

The month of April gives the most striking diagram of the twelve. It displays the superposition of two practically complete dodekagons, one a large one, completing its cycle from 6 A.M. to 6 P.M., the other a small one, for 6 P.M. to 6 A.M. The resultant wind for the whole day is very nearly southeast, and practically remains so for all the months of the year, the monthly variation of resultant wind being confined to a change of velocity from about fourteen miles per hour in May to about twenty-one miles per hour in September.

If, instead of combining the south and east components to form a vector diagram, we plot their variations separately, the semi-diurnal variation in each is plainly marked; and the calculation of its constants shows that its amplitude is about three quarters of a mile per hour both in the south and rather less in the east component. The easterly increment has its maxima at 10 A.M. and 10 P.M., and at these hours the phase of the variation of the southerly component is nearly opposite. Thus, to correspond with the semi-diurnal variation of pressure, there is a semi-diurnal variation in the trade wind at St. Helena, which is equivalent to the superposition upon the resultant wind of a northeasterly component of about one mile

per hour amplitude, with maxima at 10 A.M. and 10 P.M., the hours when the ellipsoidal deformation of the spherical atmosphere is passing over the locality.

I have only dealt with one month. I believe that when all the results that flow from this simple statement can be put before you, you will agree with me that the argosy which the Meteorological Council sent out in 1891 has indeed richly come to harbor.

Let me digress to say a word in illustration of the principle I laid down that, if one would avoid waste in meteorological work, the observations must be examined forthwith and so far discussed that any ambiguities may be cleared up.

After some years of wear at St. Helena the persistent rubbing of the southeast part of the spiral metallic pencil upon the metallic paper wore away the metal and left a flat place. This got so bad that the instrument had to come home for repairs, and when it was set up again, after a year's absence, the average direction of the trade wind differed by two points from the averages of most, but not of all, of the previous years. So far as we know, the orientation has been attended to, as before, and yet it is hardly possible to resist the suggestion that the anemometer has been set slightly differently. We are now making very careful inquiries from the observer; but, in the meantime, it seems to me that there is a great opportunity for a competent mathematical physicist to help us. Dynamical explanations of the trade winds have been given from the time of Halley. Let me offer as a simple question in the mathematical physics of the atmosphere whether a variation of two points in the direction of the southeast trade wind between the years 1903 and 1905 can be regarded as real, and, if not, which of the two recorded directions is the correct one?

It would be appropriate for me to add some words about the results of last year's work upon the upper air, in which we have had the valuable cooperation of the University of Manchester. These results have disclosed a number of points of unusual interest. But we are to have an opportunity of considering that subject in a discussion before the section, and I need not deal with it here. I must, however, pause to give expression of the thanks of all meteorologists to Professor Schuster for his support of the Manchester University station at Glossop Moor. I may remind you that this generous contribution for the advancement of science on the part of Professor Schuster is in addition to the foundation of a readership in mathematical physics at Manchester and a readership in dynamical meteorology, now held by Mr. Gold at Cambridge.

I have said enough to show that the speculative ventures of official meteorologists are not all failures, and I will only add that if any mathematician or physicist would like to take his luck on a meteorological argosy he will be heartily welcomed. Part of the work will be drudgery; he must be prepared to face that; but the prospects of reaching port are reasonably good, so much so, indeed, that such a voyage might fairly lead to a claim for one of the higher academical degrees.

Up to now I have been dealing with the adjustment of official scientific work to reduce waste to a minimum, in so far as it lies within the control of those responsible for an office. I turn now to an aspect of the matter in which we require the assistance of others, particularly of the British Association.

The most serious danger of waste in a busy office is that it should carry on its work without an adequate knowledge of what is being done in advancing science and improving methods elsewhere. I speak myself for the Meteorological Office

alone, but I believe that the responsible officials of any scientific government department will agree with what I say.

Year by year some Timotheus "with his sounding flute and tuneful lyre" performs some miracle by the application of reasoning to the phenomena of nature. Only last year you heard Professor Love in his presidential address treat of the mundane question of the shape of the earth and etherealize the grim actualities with the magic of his spherical harmonics. Year by year, in every one of the subjects in which the practical world is immediately interested, active students, whether public officials, academic officials, or private enthusiasts, not only keep alight the sacred flame but occasionally add to its brilliance; and all the new knowledge, from whencesoever it comes, ought to be applied to the service of the state.

The actual volume of original contributions on these subjects is by no means inconsiderable. You are all aware that, some years ago, the Royal Society initiated a great international enterprise for the compilation of a catalogue of scientific literature. I have been looking at the fifth annual issue of the volume on "Meteorology," including "Terrestrial Magnetism." I may remark that the catalogue is quite incomprehensibly eclectic as regards official literature, but let that pass. I find that, in the year that closed with July, 1907, 1,042 authors (not counting officers and institutions as such) presented to the world 2,131 papers on meteorology, 229 on atmospheric electricity, and 180 on terrestrial magnetism. This will give some idea of the annual growth in these subjects, and may convince you that, after all allowance is made for duplicate titles, for papers of no importance, and for mere sheets of figures published for purposes of reference, there remains a bulk of literature too large for any single individual to cope with if he has anything else to do.

If instead of confining ourselves to what can be included in meteorology alone we extend our view over the other allied sciences, it would be necessary to take in other volumes of the international catalogue, and there would be some overlapping. I have taken instead the volume of the *Fortschritte der Physik* for 1906, which deals with "Kosmische Physik." It is edited by Professor Assmann, who adds to his distinction as head of the Royal Prussian Aeronautical Observatory of Lindenberg that of an accomplished bibliographer. In this volume are given abstracts or titles of the papers published during the year which can be regarded as worthy of the attention of a physicist. An examination of the volume gives the following numbers of the papers in the different sections:

		Papers
Astrophysics		222
Meteorology		1,122
Atmospheric electricity		135
Geophysics:		
Geodetics	105	
Seismology and volcanic phenomena	256	
Terrestrial magnetism and aurora	108	
Currents, tides and waves	46	
Inland hydrography	117	
Ice, glaciers and ice age	139	
Other papers	126	
		897
Total		2,376

I need hardly say that these 2,376 papers are not all English; in some of the sections few of them are in that language, and fewer still are British. If British students, official and unofficial, are to make the most of the operation of drawing the angels down, they need help and cooperation in dealing with this mass of literature, in winnowing the important from the unimportant, and in assimilating that which makes for the real progress of

the practical application of science. This is the more necessary for these subjects because there is no organized system of academic teaching, with its attendant system of text-books. In a subject which has many university teachers it might reasonably be supposed that any important contribution would find its way into the text-books, which are constantly revised for the use of students; and yet, in his presidential address to the Royal Society in the November of last year, Lord Rayleigh felt constrained to point out that, for the advance of science, although the main requirement is original work of a high standard, that alone is not sufficient. "The advances made must be secured, and this can hardly be unless they are appreciated by the scientific public." He adds that "the history of science shows that important original work is liable to be overlooked and is, perhaps, the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston and B. Stewart will suggest themselves to the physicist, and in other branches, doubtless, similar lists might be made of workers whose labors remained neglected for a shorter or longer time."

If this is true of physics how deplorably true it is of meteorology. If I allow a liberal discount of over 50 per cent. from the numbers that I have given, and estimate the number of effective contributions to meteorology as recognized by the "International Catalogue" at a thousand, which agrees pretty well with that given by the *Fortschritte der Physik,* and if I were to ask round this room the number of these papers read by any one here present, I am afraid the result would be disheartening. Many of us have views as to the way in which the study of meteorology ought to be pursued, but the views are not always based on an exhaustive examination of the writings of meteorologists. Few of us could give, I think, any reasonable idea of the way in which it is being pursued by the various institutions devoted to its application, and of the progress which is being secured therein. Meteorological papers are written by the hundred, and, whether they are important or unimportant, they often disregard what has been already written in the same or some other language, and are themselves in turn disregarded. I do not think I should be doing any injustice if I applied similar remarks to some of the other subjects included in the table which I have quoted. How many readers are there in this country for an author in terrestrial magnetism, atmospheric electricity, limnology or physical oceanography? But, if the papers are not read and assimilated, the advancement of science is not achieved, however original the researches may be.

By way of remedy for the neglect of important papers in physics Lord Rayleigh suggests that teachers of authority, who, from advancing years or from some other reason, find themselves unable to do much more work in the direction of making original contributions, should make a point of helping to spread the knowledge of the work done by others. But what of those subjects in which there are no recognized teachers? and in this country this is practically the case with the subjects which I have mentioned. It is true that many of them are made the occasion of international assemblies, at which delegates or representatives meet. But such international assemblies are of necessity devoted, for the most part, to the elaboration of the details of international organization, and not to the discussion of scientific achievements. The numbers attending are, equally of necessity, very restricted.

The want of opportunity for the discussion of progress in these sciences is spe-

cially lamentable, because in its absence they lose the valuable assistance of amateur workers, who might be an effective substitute for the students of an academic study. In no subject are there more volunteers, who take an active part in observing, than in meteorology; but how few of them carry their work beyond the stage of recording observations and taking means. The reason is not lightly to be assigned to their want of capacity to carry on an investigation, but far more, I believe, to the want of knowledge of the objects of investigation and of the means of pursuing them.

Among the agencies which in the past have fostered the knowledge of these subjects, and stimulated its pursuit, there stand out prominently the annual meetings of this association. It was the British Association which in 1842 re-founded the Kew Observatory for the study of the physics of the atmosphere, the earth, and the sun. It was the British Association which promoted the establishment of magnetic observatories in many parts of the earth, and in the early sixties secured the most brilliant achievements in the investigation of the atmosphere by means of balloons. I know of no other opportunity of anything like the same potentialities for the writers of papers to meet with the readers, and to confer together about the progress of the sciences in which they are interested. But its potentialities are not realized. Those of us who are most anxious for the spread of the application of mathematics and physics to the phenomena of astronomy, meteorology and geophysics have thought that this opportunity could not properly be utilized by crowding together all the papers that deal with such subjects into one day, or possibly two days, so that they can be polished off with the rapidity of an oriental execution. In fact, the opportunity to be polished off is precisely not the opportunity that is wanted. There are some of us who think that a British Association week is not too long for the consideration of the subjects of which a year's abstracts occupy a volume of six hundred pages, and that, if we could extend the opportunity for the consideration of these questions from one or two days to a week, and let those members who are interested form a separate committee to develop and extend these subjects, the British Association, the country and science would all gain thereby. I venture from this place, in the name of the advancement of science, to make an appeal for the favorable consideration of this suggestion. It is not based upon the depreciation, but upon the highest appreciation of the service which mathematics and physics have rendered, and can still render, to the observational sciences, and upon the well-tried principle that close family ties are strengthened, and not weakened, by making allowance for natural development.

The plea seems to me so natural, and the alternatives so detrimental to the advancement of science in this country, that I can not believe the association will turn to it a deaf ear. W. N. SHAW

GRAPHIC ART IN SCIENCE[1]

As a beginning I wish to thank this society for the privilege granted me to address it. That a strong personal pleasure is felt at this opportunity, I shall not attempt to deny; but I have a greater satisfaction than this, and that is that I am allowed to present a subject which has been too seldom publicly discussed in the presence of investigators and students, viz.: the part played by graphic art in science.

This subject is one of growing importance, and I trust I shall live to see the day

[1] Read before the Harvey Club, University of California, November 7, 1907.

when every student of the natural sciences will be obliged to take a thorough course in drawing as adapted to his own special requirements. And I hope, as well that this course will be so arranged that the student will understand at least the *theory* of the several processes by which the printer reproduces a drawing. In fact, I wish this address to be considered as a stroke toward that end.

Just when the first drawing was made to illustrate an idea, of course, can not be determined. Perhaps it was nothing more pretentious than a simple sketch, fashioned with a sharpened twig upon some smooth bit of sand. From such a small beginning, however, graphic art has developed prodigiously, and to-day it forms a large integer in nearly every branch of science. In view of this, almost every university of repute, both in this country and abroad, employs a salaried scientific artist. At Johns Hopkins, in Baltimore, where is situated perhaps our best medical college, an actual corps of expert artists is kept busily employed in illustrating scientific articles there produced.

But this growth has not been unattended with set-backs. Graphic art has not won its place in scientific work without many a bitter struggle. In truth, even at this late date there are, unfortunately, many investigators doing exceptionally good work, who do not appreciate the highest type of drawing, and actually refuse to include it in their publications.

This folly can not be attributed to any personal aversion entertained towards fine art, it is simply the result of ignorance. These individuals have not been properly trained to see the good points in realistic drawing, and, not being by nature of an artistic trend, they simply refrain from adopting something they can not understand. As an example of this occasional "tiff" between realistic and diagrammatic representation of scientific work, the following experience of a student with a "hard-headed man of science" might be cited:

All the drawings made in the zoological laboratory of this particular school were being done in black-and-white, but the student felt that if drawing has any value at all in the study of zoology, it should be to direct the observer's attention to *all* the features of the object examined—its *color* as well as its *morphological* characteristics. Accordingly, he brought a color box with him one day into the laboratory, and started to draw a certain preparation with as near the same color-variations presented as his talent permitted. This drawing soon attracted admiring glances from the other students engaged in the laboratory. When it was about completed, the principal of the school, the "hard-headed man of science" approached the "realist," and said with some asperity: "This is not an art-academy, this is a zoological laboratory. You had better make your drawings in simple black-and-white." Had the astonished student been behind in his work, had his respect for realism produced a bad effect on the decorum of the class, or had his drawing not been the best in the laboratory, I think he would have been honest enough to have taken the principal's advice. But none of these things were so. In consequence he thought the excellencies of his product sufficient justification for its novelity, and, without laying down his brush, he answered respectfully but firmly: "If science does not approve at present of life-like illustrations, the day *must* come when it will!" The principal made no reply and walked away with a frown. The student found out afterwards, however, that he was no innovator, as very excellent scientific drawings in color had been made a good hundred years before that old fellow was

born. But the old man has never changed his view-point, and to this day a schematic drawing, done in sober black-and-white, remains the only kind of scientific illustration that he can tolerate. And yet he has done some very good investigative work, is an excellent educator of considerable reputation and has been successful in the financial side of life. So, he can not be counted below par in mental attributes. He was simply not properly trained to appreciate the value of realistic drawing. Let us guard against such a poverty of understanding in ourselves, and, by word and example, help root it out of others with whom we come in contact.

It is hardly necessary to defend the practise of drawing as an adjunct toward explaining an idea, as surely every one here has convinced himself of that fact long ago, even while reading an ordinary daily newspaper. And much more welcome is an illustration in some complex scientific discussion, where words alone so often fail completely to tell the story, or merely give a faint outline of what the observer wishes to convey. But, until quite recently, the practise of drawing was not accorded its deserved recognition in the *teaching* of scientific subjects. What physician of fifty years ago was obliged to systematically draw the bones he studied? Or, what one of thirty years ago was obliged to draw the preparations he investigated with the help of high-power lenses? Comparatively, they were good men, it is true, but one can always improve, and at this time there is hardly a medical school of any merit where drawing is neglected as part of the routine laboratory work, and this is a very happy condition, indeed, for it exacts the closest scrutiny of objects examined by the student. His attempt to picture on paper some particular object reveals characteristics of that object which would assuredly have escaped his observation if its graphic illustration were not exacted.

But, in spite of this general recognition accorded to drawing as a teaching method, as far as I can learn, there is not a single text-book or laboratory guide of any practical completeness in microscopical drawing, and this is only one division of graphic art as employed in the scientific world. Students in the laboratory are told to get this pen or that, this grade of pencil, such and such a kind of ink and paper. Perhaps blenders, erasers and thumb-tacks are alluded to. But, aside from this, the beginner seldom receives further instruction as to how he is to go about the difficult task of drawing what he sees through the microscope. His looks and actions often show how much he would welcome any advice concerning a method of approaching this new work. He is alone in a strange country without a road in sight.

Feeling that something should be done to contend against this unfortunate state of affairs, I determined to prepare a short description of some materials and methods commonly used in the drawing of microscopical preparations, and Professor Hardesty has thought this sufficiently worthy to give it a place in his "Laboratory Guide in Histology."

The condition should prevail, however, that when a student has brought his career so far forward as to be admitted to the study of medicine, he should then be trained already in microscopical drawing. He has so much to do in this highly complex problem that the extra time spent in acquiring an illustrative technic is given at the expense of more important matters.

Let us now drop the student and his troubles for a time, and turn to the field where graphic art finds its most dignified and important employment in the sciences, that is, in the illustration of investigative and teaching publications.

For its practise in these spheres, allow me to emphasize that no one should be chosen except talented and well-trained artists. It is to be lamented that such artists are not made use of to any great extent to-day. A merely superficial acquaintance with some otherwise very good modern text-books and current scientific literature is quite sufficient to prove this statement. And, at a glance, it is rather difficult to understand why such should be the case. In fact, a number of obstinate conditions contribute to the matter. First of all, only in rare instances has the investigator the adequate financial support at his disposal to pay for the services of first-rate artists. Secondly, under the existing conditions first-rate artists can not be supplied in fitting numbers to fill even the available positions. And thirdly, the investigator is too often one who underrates, or does not appreciate, the value of realistic drawing.

Of these obstacles perhaps the financial one is the most common and difficult to contend against. Scientific text-books are used by such a comparatively small public that the publisher is compelled to cut down the expenses of their production to the lowest allowable figure. Otherwise his limited profit would be lost. And, with this economy, the illustration of text-books is very likely to suffer most. Good artists must be fairly well paid, and, to retain the merits of good drawing, somewhat expensive methods and a fine grade of paper are necessary in the reproduction by the printer. To raise the price of a book to any considerable degree, because of the extra expense its illustration has entailed, is apt to be risky, for already the student and investigator groan aloud under the price they are obliged to pay for additions to their professional libraries. And yet, knotty as it seems, much can be done towards solving this problem, and the best way to commence the task is by employing only *competent* artists. A skilful artist can tell a story more convincingly with fewer illustrations, and in a technic which will allow a cheaper method of reproduction, than an artist whose talent is but moderate and whose training has been mediocre. A second-rate artist is apt, as well, to be a slow one. Not alone does he waste time in the actual drawing of the object, but it is only too often the case that the investigator or editor is forced to spend considerable valuable time in explanation before an inferior artist grasps the idea to be conveyed by an illustration. Occasionally, even then one is not through with this type of artist, for, while he may have seemed to understand what was wanted, he will return with a drawing that is entirely erroneous, and the whole trying performance must be gone through with again. Thus, economy in the matter of artists frequently amounts to nothing but unpardonable extravagance.

Leaving text-books and their problems, let us turn attention to the vehicles through which investigators disseminate their findings. These journals are, for the most part, conducted by scientists, and there is seldom the hope among them that the journal will bring in more than enough to pay for its publication. So, profit falls out of the argument entirely, with sometimes a consequent increase in the facilities for elaborate illustration. It is only now and then the case that an editorial staff of such a journal will balk at any reasonable expense necessary to reproduce the drawings explanatory of the text in a piece of research. Accordingly, aside from the limited funds at the investigator's command, there is hardly any reason left why the very best artist should not be employed to illustrate his work. Most of the institutions, which exist merely for the furtherance of science, though running at about the utmost limit of their financial resources, consider the

occasional use of an artist, perforce, as a sort of necessary evil. But the little money at disposal for his pay oftentimes precludes the services of a capable and experienced man. Yet, if the employment of inferior artists in the production of text-books is, in the end, a form of extravagance, it is doubly so in the illustration of purely investigative labor. The very fact that the observer so frequently calls in graphic art to assist in his presentation of a study, proves that he feels there are certain clefts in the description of this study which words can not bridge over. He needs an *optical* picture. If this optical picture is not just as he sees it in his preparations, there is bound to be a conflict between it and his text, which is bound to confuse rather than enlighten the reader. Therefore, the *best* drawing should be utilized, and *only* the best. But again comes the discouraging complaint: "How is the best drawing to be obtained when sufficient funds are not at hand to employ a good artist, and with hardly enough for a poor one?" This condition will exist until the investigator throws aside his lukewarm attitude in the matter, and takes up an earnest campaign towards furnishing the necessary money for the employment of his indispensable ally, viz.: the finished scientific artist.

I can recall instance after instance where an investigator has worried through weeks and even months of vexation with an inferior artist, and, as soon as the illustrative work was completed, forgotten all about his trials until it became necessary to call upon the illustrator for the needs of another publication. In the period of suspended graphic activity no attempt was made by the investigator, or at best, only a feeble, abortive one, to supply a competent artist. Every facility, on the other hand, would be called into play to supply the laboratory paraphernalia requisite to the proper pursuance of investigative procedures, but the artist, secondary only to the text in the publishing of this investigation, had been almost completely overlooked. Now who is to know about this desired help if the individual who needs it most does not give it expression? A faculty is not to be censured if it does not take steps towards allowing each research laboratory under its dominion the financial means to emply a capable artist, if the director of that laboratory has not vigorously presented his need of one. Granting that the available funds of an institution are not plentiful enough to allow the installation of a trained scientific artist in any one or in the different departments of investigation, the heads of the departments should see to it then that no opportunity passes to bring this keenly felt want before the consideration of wealthy, public-spirited individuals who are able to contribute towards its dissipation.

With the establishment of paying positions, and schools for their proper training, to be described later, there can be no doubt that the number of skilled, scientific artists would rapidly increase. Further, the number of scientific artists who, at present, do not understand the immense advantages offered by a high type of realistic graphic art would speedily diminish by the coming in contact with those who are able and determined to practise it.

In the preceding remarks, frequent reference has been made to realistic graphic art, or realistic drawing. These terms should be self-explanatory, but, not wishing any confusion to arise, a fairly exhaustive discussion of them will be hardly superfluous.

Let us suppose that nothing more complex is to be represented than an ordinary bottle, standing on a table and at some distance from, although in, the unob-

structed light of a window. It is quite true that any one would know what sort of an object the artist intended to picture, if only the correct, bare outlines of this bottle were drawn. These outlines would suggest a bottle, nothing but a bottle, yet with very little of its special characteristics. But an outline drawing of it would not be a type of realistic drawing, for even the most imaginative person could not find any similarity between the bottle and the drawing, save in the outline. Now suppose that the outline drawing be filled in with the proper shading to give it the appearance of being round, a step towards realism would therewith have been taken; but, in spite of that, the drawing is still far from being a good example of graphic art. Comparing it with the bottle in a critical way, one could not honestly and irrefutably identify one with the other, for, as example, this shading, directed alone towards showing that the bottle is round, does not develop the fact that the bottle's surface is highly polished and hence *reflective*. Continuing, however, the subtleties of shading to the degree where the light reflected from the table on to the bottle, and *vice versa*, is shown, to where the source of light, namely, the window, with its typical cross sashes, is seen in miniature upon the smooth glass, and to where other characteristics have been added, the drawing has at last reached the fullness of realism, it has become, in a manner, a *portrait*. Any one now would recognize at a glance what *particular* bottle the drawing was intended to represent, providing, of course, that one compared it with the bottle under the same conditions of position and lighting.

This very humble subject, a smooth, highly polished bottle, has been taken to illustrate realistic drawing, since its characteristic of reflection is shared by some forms of laboratory specimens subjected to certain conditions. Yet, the greatest scientific artist in this country has been severely criticized by some investigators because, at times, he allows his source of light, a near-by window, to appear in his drawings, as it clearly did on the reflective surfaces of the objects drawn.

There is a common enough belief in scientific circles that any feature of a drawing not directly contributing to the main idea which an investigator wishes to express, must be looked upon as a blemish. And this is true to some extent. But the main idea seldom exists severely alone, unsupported by secondary ones. As a matter of fact, in nearly all instances, a main idea might be considered as an accumulation of associating secondary ideas. The particular artist mentioned above realizes this truth, and for those who share his belief, the delineation of secondary ideas never disturbs, but, on the contrary, markedly assists in his forcible expression of the central thought. To cite an example:

Turning to a certain page of a recent, scientific text-book, we find a drawing of a cyst by this particular artist whose realism has been, as remarked, criticized by a number of competent investigators. In the text describing the cyst, from which this drawing was made, the author states that its surface was very smooth and moist. For the benefit of those who do not know what a cyst is, it might be generally described as a closed sack, more or less globular in form, its walls consisting of organic matter, and filled with more or less fluid contents. Any surface, whether organic or inorganic, if it be *very smooth* and *moist*, is bound to be *reflective*. Now, whether this surface reflect the source of light, to which it is subjected while being drawn, or some feature of the plane upon which it rests, or some other near or distant object, is quite a

matter of choice; but some recognizable thing must be pictured on the surface to show that it is reflective, if in a high degree. This was the case with the cyst described by this particular author and drawn by this particular artist. Now why should the artist be criticized because he chose his source of light, the window, as the object reflected, providing, of course, that it did not obscure any structural detail described by the author? In this instance it did not obscure. By drawing in his source of light, the window, since he had to do with a highly reflective surface, he simply introduced a secondary fact which largely contributed to, without detracting from, the main idea, and he added thereby only a touch of *realism* to his picture.

Now you may ask why has so much stress been laid upon this *realism* as applied to graphic art in science? The answer to that question may be begun with another question—Why is there such a pursuit as scientific investigation? Surely one of its chief reasons for existence is to dispel the mystery that often overclouds even the simplest phenomena of life, and to lead us, if not into the full, white light of complete understanding, at least, into the light of partial comprehension. And often when one seeks to describe some findings that have resulted from his exploration into the unknown, he finds that his words are too vague and inadequate to present the complete and correct description. He desires to convey to others a delicately accurate idea of the object as he sees it, and naturally must turn to the use of a picture. Therefore, an investigator should never tolerate a drawing illustrative of his text that does not vividly contribute to express, in an optical sense, completely and truthfully, the object described.

But, even realism can be carried too far in the drawing of scientific subjects, and to avoid this a fine discrimination is necessary: a discrimination that the artist can not be expected to exercise if he does not know *what* he is drawing, and, more important, the *purpose* of the drawing. This leads us to what I consider the most momentous matter in this address, and that is a discussion of the ideal relationship which *should* exist between scientist and artist, and the means by which this might be effected. At present it is only too common that inharmonious relations exist between scientist and artist.

In the first place, an individual whose nature has led him into a life devoted to investigation is seldom an adept in the use of tools employed in the practise of graphic art. At times it seems that investigative talent, and the talent necessary to worthily render anything in a graphic way, are natural antagonists, so rarely are they found inculcated in the same person. But there are very few good scientists who, if they choose and have the opportunity, can not become excellent critics of the good and bad points in a drawing or painting, especially when it is used to illustrate subjects with which their business in life has made them thoroughly familiar.

And, on the other hand, very few artists display any taste for or merit in work of a purely investigative character. Most of them are loath to believe that any high art can be worked into subjects that present a pattern of almost infinite detail. But this is not all. Without the proper guidance, even an eager, capable artist set upon the task of drawing some scientific subject, would be, in nearly every instance, sure to give some important feature of the same only an indifferent expression, or omit it entirely, while another one, perhaps an artifact, something that a horde of accidental conditions might produce and which is entirely foreign to the subject in its natural state, would be painstaking and conspicuously copied, thus rendering his production

absolutely useless to the scientist. So the scientist with little artistic talent, and the artist with no scientific training, clash together again and again, leading to a loss of precious time, and a severe straining of patience on both sides. This thundercloud hangs over both until, by constant rubbing together, each takes on some of the other's particular efficiency. Under the conditions existing to-day, this is a slow and laborious process, and frequently attended with exhibitions of the most fretful behavior on the part of both principals. The scientist gets angry with the artist, and *vice versa,* each consoling himself with the firm conviction that the other is a microcephalic egotist. And this is not so strange as at first appears when we look at the methods by which each has attained his smattering of the other's specialty. The scientist, in the course of his career, because of the demands laid upon him by the publishing of his work, is, from time to time, brought into brief and, as a rule, imperfect contact with the processes by which a drawing or painting is reproduced in printed literature. He hears about light and shade, color and tone values, contour and perspective, half-tone and auto-type, etc., until he has learned them by heart, and only with a ghost of an idea what they all mean. Hence, he can not be a judge of what particular process of illustration best adapts itself to the subject described, and when a skilled artist points out the respective merits of different processes of delineation and reproduction, he finds that his words fall on, at best, only partially comprehending ears. Yet the artist may be just as far in arrears in his scientific obligations. It is a long, hard struggle for him to suppress his realism to the interest of the scientific purpose of an illustration. Time and time again he spends fond hours with some particular thing he sees in the preparation, only to have it ruthlessly cut out by the investigator as a positively unnecessary and confusing feature in the drawing. And it is no uncommon occurrence to see an artist presenting himself for service in scientific circles, boasting of nothing more than a bowing acquaintance with the microscope. He looks into it with a squint, sends the oil-immersion lens down through the cover-glass, and carries the instrument about by its delicate adjusting-mechanism.

Coming to the question of what should be done to improve the relations existing between scientist and artist, the terse advice of Max Brödel sums up the whole problem—"Teach the scientist more art and the artist more science." And how is this to be done? I can see but one logical way, and that is by modifying and establishing courses where both scientist and artist may be trained in a proper and thorough manner concerning what each should know of the other's occupation. From the best available sources of information I am told that such a school is nowhere in existence to-day. Here is an opportunity for this university to take up an initiative which it would never need to regret. For, with the proper management, I am sure that such a course of instruction would be nearly, if not quite, self-supporting, and the good results to be obtained from it would earn the hearty thanks of every branch of scientific work coming within the pale of its influence. This is not the time or place to discuss the equipment or conduction of such a course of study. Suffice it to say that neither would demand heavy financial expenditure. While the mature scientist and mature artist would have, naturally, recourse to the training here offered, the greatest bulk of its attendance would be from the ranks of the beginners

in both pursuits. The students of natural sciences could be here enrolled in their several classes, and instructed in the graphic arts as demanded by their own special needs. And individuals of graphic talent, those intending to follow the illustration of scientific subjects as a life-work, could be here instructed in a manner calculated to amply fit them for the graphic execution of any subject in any branch of scientific investigation. At least two courses, experimental and didactic, would be attended by both sections of workers, viz.: reproductive methods, that is, the various processes by which a drawing or painting is published, and a systematized explanation of that ideal relationship which *must* exist between scientist and artist to insure the best results. The mere mention of these two courses calls up to mind a generous number of subjects embraced by them, each one of which, if anything like justice were done it, would require more time than is allowed to any single period of discussion. But, and I have no doubt at all as to the truth of this assertion, every one, be his help great or small, who assists towards raising the institution from the *suggested* to the *realized* will have done every form of scientific work a great service.

In closing this effort towards bringing art and science into closer, more effective affiliation, I could hardly find a higher sentiment, or an example of nobler support of this cause, than that expressed in a recent personal communication from Mr. Max Brödel, the most capable of all the artists engaged in illustrating American science to-day. They run as follows:

> The only course on the subject of medical illustration is given by myself, and is limited to a few individuals. I don't wish to leave this world without having done some good, and I believe I can help the beginner in the study of medical illustrations to avoid a great many pitfalls and disappointments. There is not a penny in it for me, but I do it because I think it is my duty towards the medical profession.

A. W. LEE

UNIVERSITY OF CALIFORNIA

THE MARINE BIOLOGICAL STATION OF ROSCOFF. ANNEX OF THE UNIVERSITY OF PARIS

A CIRCULAR received some time ago from Professor Delage concerning the Marine Laboratory at Roscoff, seems worthy of reproduction in SCIENCE in order that the advantages of the station may be brought to the attention of American students who may be intending to pursue zoological studies abroad.

J. PLAYFAIR MCMURRICH

Founded in 1872 by H. de Lacaze-Duthiers, this station had at its beginning only a rudimentary equipment. But the judicious selection of its site was a certain guarantee of its ultimate development, for in its immediate neighborhood all varieties of sea bottom are to be found, with the fauna and flora characteristic of each; indeed, there are few points, either in France or abroad, that can compare with it in the richness and variety of their fauna. In addition the tides are very high (almost 10 meters) and expose a large extent of shore, so that the collection of quantities of material is very easy.

The progress of the station has been continuous and to-day it is a vast establishment whose buildings cover the extent of a demi-hectare and comprise a large aquarium with 300 square meters of floor-space and containing 20 aquaria and 2 large basins, all supplied with a constant circulation of sea water; an aquarium of nearly 1,000 square meters of surface and 4 meters in depth, supplied by the tide; elevated tanks of 180,000 liters capacity for the supply of the large aquaria and the smaller ones in the work-rooms; 22 work-rooms for students carrying on original researches; a museum containing examples of all the animal forms of the region, identified by specialists; a large hall for students, in which two courses of lectures and laboratory work, each consisting of thirty sessions, are conducted; a library; a physical laboratory; a chemical laboratory; two photographic rooms; an engine room; a workshop; and twenty living rooms for investigators who may find it necessary to live in close proximity to their work.

The equipment also includes several small sail-

boats and a motor boat of eighteen tons, in which excursions may be made as far as the English coast.

The number of workers in attendance at the station, which in former years has varied between thirty and forty, has doubled, and it has been found necessary to make considerable enlargements in order to accommodate all those who request the hospitality of the station.

The aquarium and the old work-rooms have been pulled down and reconstructed on better plans, and 24 new rooms have been added, all lighted by large windows and supplied with all the scientific equipment which modern technique demands. These rooms are intended as private laboratories and are placed at the disposal of students from abroad at an annual rental of 1,500 frs., payable to the faculty of sciences of the University of Paris. This charge is notably less than that made by the Zoological Station at Naples. It must be understood, however, that the Roscoff Station makes no pretensions of duplicating that at Naples. Situated as it is on the shore of a tidal sea, whose fauna is quite different from that of the Mediterranean, it is, on the contrary, a natural complement to the more southern station.

THE BOGOSLOF ISLANDS

The following notes have been received from the North American Commercial Company, of San Francisco, under date of June 30, 1908:

Our Dutch Harbor log contains the following: "Wednesday, June 17th, 1908: The *Rush* passed here for Unalaska at three P.M. from the Seal Islands. Captain Munger returned on the *Rush*. He went up to the islands on the *McCullock*. On the way down the *Rush* had intended doing a little surveying around Bogoslov, but the navigating officer could not find the islands." The company's letter of the 17th instant from St. Paul Island says: "The *McCullock*, returning from Bogoslov, reports the disappearance of McCullock and Perry peaks. A reef adjoining Castle Rock now forms a small bay. We are not in possession of all the facts."

It will be remembered that the Bogoslofs are a group of three small volcanic islands in the southern part of Bering Sea and thirty-seven miles northwest of the island of Unalaska. One of these islands, Castle Rock, has been known since 1796, Fire Island has been known since 1883, while the third one, Perry Island, rose from the sea about the time of the San Francisco earthquake in 1906.

There is a brief account and photographs of these islands published by Dr. Jordan and Mr. G. A. Clark in the *Popular Science Monthly* for December, 1906, pp. 481–489.

J. C. Branner

Stanford University, Cal.,
September 17, 1908

SCIENTIFIC NOTES AND NEWS

The Academy of Sciences at Turin has awarded its Riberi prize of the value of $4,000 to Professor Bosio, of Turin, for his discoveries in relation to the biological reactions to arsenic, tellurium and selenium.

Sir James Dewar, Fullerian professor of chemistry at the Royal Institution, London, and Professor O. D. Chvolson, professor of mathematical physics at St. Petersburg, have been elected foreign members of the Belgian Academy of Sciences.

Mr. Wilbur Wright, as has been fully reported in the daily press, has made with his aeroplane at Le Mans a flight lasting one hour, thirty-one minutes and thirty-five seconds and covering 66.6 kilometers. He has also made a flight with a passenger lasting 55 minutes and 30 seconds and covering 58 kilometers.

Dr. Luthur H. Gulick has resigned the directorship of physical training in the New York public schools to become secretary of the Physical Training Department of the National Young Men's Christian Association.

Dr. S. Tscherny, of Kiev, has been appointed director of the university observatory in Warsaw.

Mr. Robert Nelson has been appointed to the newly created post of electrical inspector of mines in Great Britain.

Professor L. H. Bailey has been given leave of absence from the directorship of the College of Agriculture of Cornell University to devote his time to the chairmanship of the commission appointed by President Roosevelt to investigate the conditions of rural life. He expects to be at Ithaca during the present month and in Washington during November

and December. The other members of the commission are: Henry Wallace, of *Wallace's Farmer,* Des Moines, Ia.; President Kenyon L. Butterfield, of the Massachusetts Agricultural College; Gifford Pinchot, chief of the Forest Service, and Walter H. Page, editor of the *World's Work.*

DR. N. L. BRITTON, director-in-chief of the New York Botanical Garden, and Mrs. Britton have returned from a fourth trip of botanical exploration of the island of Jamaica.

PROFESSOR H. E. CRAMPTON has returned to Columbia University from zoological explorations in the Society Islands.

DR. GEORGE N. STEWART, professor of experimental medicine in Western Reserve University, who has been abroad on leave of absence for the past year, has returned.

PROFESSOR J. H. COMSTOCK has returned to Cornell University after a year abroad.

DR. WILLIAM OSLER has received leave of absence from Oxford University for one year, which he will spend on the continent.

PROFESSOR HENRY A. PERKINS, of Trinity College, will spend the coming year in physical research at the University of Paris.

DR. LYMAN B. HALL, professor of chemistry in Haverford College, has been given leave of absence for the present academic year.

THE expedition under Professor Baron Gerard de Geer, of Stockholm University, which has been exploring the Spitsbergen group of islands, has returned.

PROFESSOR EDWARD L. NICHOLS, head of the department of physics of Cornell University, will represent the university at the inauguration of C. A. Duniway as president of the University of Montana.

PROFESSOR GEORGE TRUMBULL LADD is now giving a course of fifteen lectures upon certain psychological aspects of education at the College for Women of Western Reserve University.

PROFESSOR CHARLES BASKERVILLE began on October 3 a series of six lectures on the chemistry of existence, to be given at the American Museum of Natural History on successive Saturday evenings.

THE Rev. Edmund Ledger, M.A., has resigned the Gresham lectureship in astronomy in Gresham College, London, which he has held since 1875.

THERE was quoted in the last issue of SCIENCE an editorial article from the New York *Evening Post* discussing academic freedom in America, which was based on the alleged call of Professor George A. Coe to the Union Theological Seminary. This article, it appears, was incorrect, both in regard to Professor Coe's call to the seminary and in regard to the alleged objections to his views at Northwestern University.

THE Royal Society of Victoria is collecting a fund to establish a medal in honor of the late Dr. A. W. Howitt, to be awarded for work in Australian natural science.

DR. HOMER TAYLOR FULLER, president of the Worcester Polytechnic Institute from 1882 to 1894, member of the Geological Society of America and fellow of the American Association for the Advancement of Science, has died at the age of seventy years.

PROFESSOR ERNEST F. FENELLOSA, curator of the department of oriental art in the Boston Museum of Fine Arts, at one time professor of philosophy in the University of Tokyo, and an authority on oriental archeology, art and philosophy, has died in London at the age of fifty-five years.

THE deaths are also announced of Dr. Theodor Peters, director of the Society of German Engineers, and of Mr. George Nicholson, formerly curator of the Royal Gardens, Kew.

THERE will be a New York State Civil Service Examination to fill the position of associate in clinical pathology in the Pathological Institute at a salary of $2,000; of director's assistant in the State Library at a salary of $2,100, and of junior statistician for the Public Service Commission at a salary of from $1,200 to $1,500.

DIRECTOR GEORGE OTIS SMITH, of the U. S. Geological Survey, has invited officers of the leading railroads of the country to a conference on the amount of water flowing in the rivers of the country under certain conditions and in different periods of the year. The

National Conservation Commission has been advised of the proposed plan and whatever results come from it will undoubtedly be considered by the commission at its joint meeting with the governors of the states, or their representatives, to be held in Washington next December.

An Australian Institute of Tropical Medicine has been established at Townsville, North Queensland. It receives subsidies of £450 from the commonwealth government; £250 from the Queensland government, and £400 from the British Colonial Office. The nomination of the first director has been delegated to the Royal Society and to the London and Liverpool Schools of Tropical Medicine.

The Virginia Geological Survey, recently established by the General Assembly of Virginia, with headquarters at the University of Virginia, is under the direction of a commission composed of Governor Swanson (chairman), President Alderman, of the University of Virginia; President Barringer, of the Virginia Polytechnic Institute; Superintendent Nichols, of the Virginia Military Institute, and Hon. A. M. Bowman, of Salem, Va. At a recent meeting of the commission Dr. Thomas L. Watson, professor of economic geology in the University of Virginia, was elected director of the survey. Dr. J. S. Grasty, of the Maryland Geological Survey, was appointed assistant geologist, and Mr. Wm. M. Thornton, Jr., of the University of Virginia, chemist. Work is in progress on the geology of the coastal plain region, including the underground water resources; cement and cement materials; topography and geology of the Virgilina copper district; geology of the rutile deposits; and building and ornamental stones. The studies of the coastal plain geology and topographic mapping of the Virgilina district are in cooperation with the United States Geological Survey.

UNIVERSITY AND EDUCATIONAL NEWS

Mr. and Mrs. E. W. Clark, of Philadelphia, have given $10,000 to Harvard University for the establishment of two freshman scholarships.

Columbia University has received $10,000 from Mr. and Mrs. Henry F. Shoemaker, the income to be used in aiding poor students.

Among the alumni of Haverford College a fund is being raised for a science hall.

With the opening of the college year on September 24, the University of Wisconsin College of Agriculture inaugurated a new course known as the middle course, which may be completed in two full years. This course is designed to meet the needs of those students who have had a high school training and are prepared for university work, but can not spend more than two years at the university. The studies include practically all the work of the first two years of the long, or four years' course, with the exception that foreign languages and mathematics are replaced by more practical subjects. The aim is to give the students as thorough training in the sciences and technical agriculture as is possible in two years time.

The Rev. W. W. Guth, A.B. (Stanford), S.T.D. (Boston), Ph.D. (Halle), pastor of Epworth Methodist Episcopal Church, Cambridge, Mass., has accepted the call of the corporation of the University of the Pacific to the presidency of that institution.

Professor H. Wade Hibbard, head of the department of railway mechanical engineering at Cornell University for the last ten years, has been appointed director of the college of mechanical engineering at the University of Missouri. He will take up his new duties the first of the new year.

Dr. Howard S. Reed, expert in soil fertility in the Bureau of Soils of the United States Department of Agriculture, has been elected professor of plant pathology in the Virginia Polytechnic Institute, and plant pathologist in the Virginia Agricultural Experiment Station at Blacksburg, Va. He entered upon his new duties at the opening of the academic year.

At Western Reserve University, Mr. Carl B. James has been promoted to be assistant professor of biology in Adelbert College. In the Medical School, Dr. Maulsby W. Blackman, instructor in histology and embryology

has been promoted to be lecturer in histology and embryology, and Dr. Roger C. Perkins, assistant professor of pathology and bacteriology to be associate professor of pathology and hygiene.

MR. CHARLES H. DANFORTH, who has been engaged in comparative anatomical investigation at Tufts College, has been appointed instructor in anatomy in the Medical Department of Washington University.

AT Northwestern University David R. Whitney, Ph.D., Columbia, has been appointed assistant in biology, and J. W. Turrentine assistant in chemistry.

AT Williams College, Mr. Charles Packard has been appointed instructor in biology; Mr. R. S. Corein assistant in geology; and Mr. L. B. Mears assistant in chemistry.

AT Amherst College, Mr. Gordon Pulcher has been appointed instructor in physics; Mr. Charles W. Cobb, instructor in mathematics, and Mr. Arthur L. Kimball, Jr., assistant in geology.

EDWARD E. WILDMAN, M.S. (Pennsylvania, 1908), has resigned a fellowship in biology at Princeton on account of his election to a professorship in biology at the Central High School, Philadelphia.

MR. P. L. GAINEY, a recent graduate of the North Carolina College of Agricultural and Mechanical Arts, has been appointed assistant in botany in that institution.

DR. JESSE H. WHITE, PH.D. (Clark), has charge of the work in psychology and education in Pittsburgh University, during the absence of Professor Edmund B. Huey, who is spending the year in Paris.

PROFESSOR M. STUART MACDONALD, of the University of Fredericton, N. B., has been invited to give assistance to the department of philosophy until the board fills the vacancy made by the resignation of Professor Taylor.

DISCUSSION AND CORRESPONDENCE

THE ADMINISTRATION AT THE UNIVERSITY OF ILLINOIS

THE abominable state of affairs which exists at some of our universities in America could not last long if the true conditions were known outside. The faculties are powerless to correct them, for the very places where reform is most needed are the ones in which the professor is so shorn of his power as to be practically helpless. If public opinion is once aroused on the seriousness of this question, it will not take long to remedy the evil.

Those who are interested in the movement for putting our American universities on a true university basis will find some valuable material in the contents of this article.

I agree with the editorial in the *Popular Science Monthly*, for July, 1908, that Dean Kent did a public service when he exposed the administration of Chancellor Day at Syracuse University. I have heard that this is already producing a better atmosphere at Syracuse—as one might have expected.

For the same reasons I propose to give an account of a case at the University of Illinois, and in doing so I have no apology to make to any one; least of all to my fellow citizens of the state or to the alumni of the university. They are the ones who are most interested, and I believe that the vast majority of them will see at once that they will be benefited.

I am trying to clean up a condition which could not exist in the light, and which will spread its poison if allowed to persist in the dark. The university is too strong, the alumni too loyal, and the state, as a whole, too intelligent to allow the recurrence of such acts as have recently been perpetrated by the president of the university, supported, in part at least, by the governor of the state.

Those who are interested in academic freedom will be edified in seeing how the president of an American university handled a professor's case when he appealed it to the supreme court of the university—that is, the trustees.

Without going into the details of the case appealed, I will simply say that when President James came to the University of Illinois, I had completed seven years as full professor and head of the department of physiology. During this entire time I had had no friction with the former president, nor with any one else.

With President James's administration I began to bump over a corduroy road that led me to believe there was something wrong under the surface. I felt that I had been compromised, in several particulars, in my dignity as a professor, and finally, having failed to get a satisfactory explanation, at the end of three years of friction, I wrote to the president suggesting that he ask my resignation rather than to keep things going as they were. I knew, and the president knew, that if he asked my resignation I could request a hearing before the trustees, and he would have to show grounds for his request. As he would do nothing, I finally asked the board of trustees to court-martial me. I was conscious of a clean record, and did not fear a fair trial. I quote the first paragraph of my letter to the board:

> When an officer in the army, by the act of a superior, is placed in a position which compromises his reputation, he may apply for a court-martial. In this spirit, I request an investigation by your honorable body.

The letter was addressed directly to the board, and was forwarded through the dean of the College of Science, and the president of the university. As evidence that I had not appealed to the board before exhausting every resource with the president, I presented to the board, at the first meeting on which the case was called, copies of two letters which I had sent to the president—one of the date of September 7, 1906; the other February 14, 1908; and I gave copies of these letters to each member who was present at that meeting. I quote from said letter of September 7, 1906, as follows:

> Although I have headed this letter "unofficial" and have marked the envelope "personal" so that it will reach you without passing through clerical hands, it will contain nothing which I wish to be guarded by the seal of privacy, and I expect you to use its contents freely: especially with others whom it may concern. I intend it for an informal discussion: a friendly *pour-parler*, so to speak; preceding official action, if such action must be taken.
>
> I am in doubt as to your attitude toward me and my department, and I wish to go straight to you for information. . . . I know of no reason to justify any action that would insinuate that I was inferior to my colleagues; and if you have any such reason in mind, I wish you would bring it up and let us have it out, fairly, fully, and finally. . . .
>
> I believe I have every moral right to remain here, and share all the benefits, in the growth of the institution to which a long term of efficient service (with many extras) would entitle me. On the other hand, if my position is to be compromised, in its dignity, either as to salary, amount of work, or responsibility, I would not be willing to retain it.

After such words as these, in a *pour-parler*, and even much more vigorous ones in an official letter, later, there was no excuse for the president to refuse to clear things up one way or the other, and the only higher court I could appeal to was the board of trustees.

When my case was called, I appeared before the board and made a brief preliminary statement, to the effect that I wished a clean slate, but that I did not wish to make a personal attack on any one, and would regret it if any personal questions arose. *I also stated that I hoped the case would reveal certain defects in our organization that I knew existed, and that I hoped the board would see and correct them.*

I was then excused, with the understanding that I would be called later. As I left the room I told where I would be, and how I could be back in three minutes if they telephoned to me.

The vice-president, the former dean and the present dean were then called. They were questioned by President James, and their testimony was taken in my absence. No record was kept.

If the president brought in these gentlemen, expecting that they would condemn me, their testimony must have been a sort of a Baalam's blessing; for at least two members of the board, who stuck by the president through thick and thin, told me that the evidence was so much in my favor, that if I would withdraw the case, I could do so without prejudice to my reputation for anything that had been said.

The board took a recess for lunch, after which they transacted other business and then

adjourned without taking up the case again. This was March 10; the meeting was at the university.

The next meeting of the board was held April 3, in Chicago. I was not notified and so was not present.

The next meeting was April 23, also in Chicago. I was notified and was present. The president of the board asked me what I had to say. I replied that I did not know what I would have to say until I was informed as to what had been done in my absence. I then said: "First, I should like to ask, 'Are there any charges against me?'" The president of the board gave me the official answer, "No." I then said, "This gives me a clean slate and leaves nothing for me to defend; but I have some witnesses who can tell of the difficulties I have had in the development of my department. I have brought these witnesses here at my own expense, and if you care to use this opportunity to learn of the conditions under which some of us have to work, I offer you their testimony." They were called in, and I voluntarily left the room to allow them to speak without the slightest restraint. They were questioned by different members of the board. Finally, the president of the board, in a genuinely courteous way, gave me ten minutes to "*close the case*"—these were his words. After I had left the room, President James, who had been present all the time, read from a nineteen-page typewritten memorandum in which he reflected on me both personally and professionally.

A resolution was then put to a vote, that the "board of trustees do not recognize that Professor Kemp has any just cause for grievance against the administration of the university." This was lost on a tie vote. Then occurred an accident which gave me the first tangible thing I could get from anybody. The president had taken the seat which I vacated when I left the room. I had left a whole stack of reports, etc., on the table; for I did not know what I might have been called on to prove, and I was prepared to show, if necessary, that my recommendations had not been carried out. After the vote went against him, the president left his memorandum mixed in with my papers, and when I gathered these up, after the meeting, I found it.

Here I had incontestable proof of the president's unfairness. He had presented this paper behind my back, and it was full of misleading statements—especially half truths, very adroitly presented. Indeed I must give the president credit (*sic*) for the way in which he put some of it together. He attacked me on practically every point which is essential to the head of a department, viz., as an administrator, as a teacher, as a man of science, and in my relation to my colleagues. Everything was general—not a specific act was alleged. Here is a sample:

> I do not think that Dr. Kemp's salary should be increased at the present time, or at all, until we get an entirely different atmosphere in the department, and particularly in the relations of the department to the other departments in the university.

If the president had said this to my face, I should have asked to which departments he referred; and what was wrong. If this *were* true, both the president and the dean had been guilty of flagrant dereliction in their duty, for not having called my attention to it. The same would apply to the head of any department making such a complaint.

I have received disciplinary letters from the (former) dean, on meeting a class out of schedule hours[1] and on "smoking in university buildings." If such trivial things as these could be put in writing, there is no excuse for secret action on one of the most serious accusations that could be brought against a professor.

Speaking of atmosphere, I believe I can say that my department has always contained as much university oxygen as any about the place, and my students who have breathed this "atmosphere" have gone out loyal, strong and true university men and women, who have made a record of which any professor may be proud. *That* is the *real* test.

Later in the day, I came upon a group of the trustees who were discussing the meeting. The reading of the memorandum behind my

[1] This was done by unanimous request of the class and involved no conflict with other classes.

back had been objected to, but the objection was not sustained. The group that I met were all indignant at the whole procedure and their talk drifted back to the meeting of April 3, for which I had received no notice.

It seems that at that meeting the governor was present, as were all the ex-officio members. The governor almost never attends even the *regular* board meetings, but he was at that *special* one. He presented a set of laudatory resolutions on the president and spoke so pointedly, on his wish that the board should back up the president, that it excited comment at the time. Shortly thereafter my case was called, and discussed in my absence. The president attacked me, then, behind my back; and tried to get the board to pass resolutions which would have led to my resignation in self-respect. Action was only deferred by the vigorous protests of some who insisted that I had been promised a *hearing;* that up to that time I had had no chance to reply to anything that had been said, and had not even heard it.

After I was sure that there was no doubt on this point, I showed those members my file of the president's letters containing this one:

University of Illinois
President's Office

Dr. G. T. Kemp, April 11, 1908.
Natural History Building

My dear Doctor,

The board of trustees at its last meeting on Friday April 3 in the city of Chicago considered at much length your communications to the president of the university and the president of the board. Through some misunderstanding it seems you did not receive notice of the meeting. I urged that action be deferred until you could be heard if you care to be.

Faithfully yours,
(Signed) Edmund J. James.

As will be seen, in my resignation, I accepted the version of the trustees—not that of the president. There is not a member of the board from the governor down who can plead ignorance after this.

There is another thing which happened at the meeting, on April 3, which has a very ugly look. I was not there and did not hear what the president said, but he certainly gave some of the trustees the impression that I had referred him to three men at Johns Hopkins, that he had seen these men, and that all three had spoken unfavorably of me. He did go to Baltimore, and if he was as misleading there, with those whom he saw, as he was in his "secret report" to the trustees, I do not know what sort of impression he created. As a matter of fact, I knew nothing of his intended visit to Johns Hopkins, and did not refer him to anybody. If I had done so I should have given him my card.[2]

The rest is soon told. The president's actions had made the whole affair a burlesque on justice; the governor had aided and abetted him, and was evidently keeping his influence active. The sense of decency of about half the board was outraged, and the others were likely to stand pat. It was disagreeable for everybody. I was disgusted and willing to sacrifice my "job" for the dignity of my profession. A university professor is entitled to more respect than to be subjected to such a farce of a hearing.

I was prepared to resign, but before I did so, the president contributed one more seriocomic feature, in the shape of a letter. It was serious, because it came from a man in his position; it was comic because of its form, and because I received a letter of the *same date,* from another official, and it would take a pretty clever casuist to make the two agree. The one letter was from the secretary of the board of trustees giving me the words of the president's resolution, which had been lost at the last meeting, and also the vote. This resolution has been quoted above. The president's letter, on the same date, is as follows:

[2] I can not pass this by, without expressing a thought which will appeal to more than one reader of Science. I have known men to have injustice done them when it was never so intended. In judging the speed of a horse, you must see if there is a handicap. A horse that can trot a mile in 3:50, carrying weight over a heavy and broken road, may have just as good stuff in him as one that can make it in 2:25 on a good track in a proper rig; and the fact that the 3:50 animal has never quit, and refuses to have his racing-spirit broken is not such a bad recommendation for his old stable.

UNIVERSITY OF ILLINOIS
PRESIDENT'S OFFICE

April 28, 1908

DR. G. T. KEMP,
Natural History Building
My dear Doctor Kemp,

The board of trustees after a long discussion over the matter did nothing in regard to the subject matter of the communications which you have forwarded to them. They will have another meeting on next Saturday at nine o'clock at the Palmer House in Chicago. If you desire to make any further statement or appear again before the board at that time I shall be very glad to ask that permission be accorded you.

Faithfully yours,
(Signed) EDMUND J. JAMES.

I do not see how the action of the board could have been more to the point. I asked a court-martial on the ground that I had been compromised by a superior. The board told me, through its president, that there were no charges against me, and put it on record. They then refused to say that I had no just cause for grievance against the administration of the university. That was as complete a vindication as any man could wish. Everybody was tired of the thing, and I felt outraged and disgusted, so I sent the president the following letter, with my resignation enclosed:

UNIVERSITY OF ILLINOIS
DEPARTMENT OF PHYSIOLOGY

May 1, 1908.

PRESIDENT E. J. JAMES,
University of Illinois.
My dear President James,

Following my letter of yesterday, I now enclose you my resignation, as the only communication I have to make to the board of trustees for their meeting to-morrow.

This settles the case so far as I am concerned, but if the board wishes me to appear, for any reason, I shall, of course, do so.

The reasons for my resigning, as I am now doing, are set forth in the preamble to the resignation. A further discussion seems superfluous.

Very respectfully yours,
(Signed) GEO. T. KEMP.

TO THE HONORABLE BOARD OF TRUSTEES
OF THE UNIVERSITY OF ILLINOIS
Ladies and Gentlemen:

WHEREAS, The Board of trustees has taken much of its valuable time in discussing matters arising from my communication, of March seventh, to the board, and

WHEREAS, The action of the board was to refuse, by a tie vote, to pass a resolution, emanating from the president of the university, to the effect that I had no grievance, and

WHEREAS, I feel that I am sufficiently vindicated, by this action of the board, to resign without compromise of my reputation, and,

WHEREAS, I do not wish to give the board any further trouble with the case, and

WHEREAS, At a meeting of the board, on April third, the president of the university urged the passage of resolutions reflecting to my discredit, and

WHEREAS, I was absent from said meeting, and, at the time the president of the university urged such action, I had been given no chance to reply to anything that had been said about me, and

WHEREAS, At the next meeting of the board, to wit, April 23, I asked the question: "Are there any charges against me?" and

WHEREAS, The president of the board gave me the official answer: "No," and

WHEREAS, I then said "May I ask that this be made a matter of record?" and

WHEREAS, The president of the board said: "If there is no objection, it is so ordered" (or words to that effect), and

WHEREAS, The president of the university was present and interposed no objection, and

WHEREAS, After the case was supposed to be closed, and I had left the room, the president of the university read from a lengthy typewritten memorandum, and made statements which reflected to my discredit both personally and professionally, and

WHEREAS, My confidence in the administration of the university is now so shaken that I think it best to resign,

THEREFORE: I hereby tender my resignation, as a member of the faculty of the University of Illinois, to take effect September 1, 1908.

Very respectfully yours,
(Signed) GEO. T. KEMP.

In conclusion, I wish to quote again from the president's secret memorandum:

Dr. Kemp makes certain definite charges involving the good faith and honesty of the university administration. I shall dismiss these with this reference, as I do not consider that it is necessary to enter into this subject at all. I call atten-

tion to it simply as an evidence of the difficulty which Dr. Kemp has in taking a proper view of university questions. Everything is so bound up with his own personal view as to what is due him and his particular department, as to make it temperamentally impossible for him to take any other but a false view of this particular phase of the subject.

In my letter to the board, I tried to avoid anything personal, and when I appeared before the board, to make my preliminary statement, I laid special emphasis on this. In fact, I gave it as my reason, for appealing to the board, that I could not locate where the trouble lay. The dean referred me to the president, and the president laid the responsibility on the former dean, the present dean, and the vice-president. I felt that I was being shot at from ambush, and when I stopped, like a man, and challenged, the president would not bring us together, but said the reports were confidential.[3]

When I found the president with his secret memorandum, I had something definite. It is the irony of fate, that in making the very flourish with which he dismissed these *alleged* charges, he dropped a paper which proved most serious things on himself in stern reality. I frankly admit that I am temperamentally so constructed that I can not regard it as either "fair or honest" for a university president to make an attack on a professor behind his back that he would not make to his face. Furthermore, the same temperamental construction forces me to feel that a man who would not look upon such an act as disgraceful does not "take a proper view of university questions," and is not the best type of man to intrust with the instruction of the youth of a nation. I do not mean these for words of passing sarcasm. The idea which they convey is of serious import to the educational interests of the country. One of the most famous educators of the past generation has said:

> No educational system can have a claim to permanence, unless it recognizes the truth that education has two great ends to which everything else must be subordinated. The one of these is to increase knowledge; the other is develop the love of right and the hatred of wrong.

If we wish our system of state education to endure, we dare not condone a serious infringement of either of these fundamental principles, for a recognized amount of ability in the money-getting or in the advertising line. The State of Illinois, for instance, is not giving nearly $1,000,000 a year in order that any man shall rear a showy structure, and say, "Behold the great Babylon which I have built." The people who furnish the money have a right to demand—and will demand—a clean administration, and a healthy atmosphere from the president's office to the athletic field. If anything half as bad as the president's attack in the dark had happened in connection with the management of the football team, there would have been a tremendous cry of "dirty athletics," and a storm of righteous indignation would have broken loose. The higher up we go, the harder it is to correct abuses—but the more important it is that these abuses should be corrected.

Geo. T. Kemp
Hotel Beardsley,
Champaign, Illinois

[3] There is food for thought here. I have some excellent friends, for whose opinions I have genuine respect, who believe that a democratic form of university government would seriously upset faculty discipline. Here we have a typical monarchical form; and what could be more subversive of faculty discipline, and of confidence, than what I have just described? I finally came to fear that the president had ulterior motives which he did not care to allege; and that he was seeking cover behind which to fight.

QUOTATIONS

AT THE UNIVERSITY OF OKLAHOMA

A third according to our informant, a fifth according to Mr. Cruce, of the faculty have been removed. They were removed with practically no notice; so late in the season that it is really a remarkable testimony to their ability that so many of them have already obtained appointments in colleges of standing not unequal to that of Oklahoma University. Accompanying this removal, without previous notice, was a refusal to pay the last two

months' salary of the year for which they were appointed. What legal justification there was for this refusal we do not know; the question is now before the courts of the state in a suit by the professors for the withheld salary. Legal or illegal, it certainly was not just. We shall require a great deal of evidence to convince us that the people of Oklahoma wish their faithful and efficient teachers treated in this fashion. For there is no pretense that they were not faithful and efficient. It is too late now to interpose charges, even if one wished to do so. Nor is it pretended that their successors are abler scholars or likely to be more efficient teachers. The value of a degree in America depends on the college or university which grants it. The men removed represent degrees from Harvard, Columbia, Johns Hopkins, Michigan and Chicago Universities. The men appointed in their place represent degrees from Harvard and Texas Universities and Coronal Institute.

No reason for the discharge of this one third or one fifth of the faculty is even hinted at by the president of the board of regents. The fact that two out of the seven members of the board present when this dismissal was voted were Republicans does not indicate that the object of the removal was not political. We do not know how those two voted; Mr. Cruce tells us that one of the two members voted against the dismissal. The fact that the Republican members voted for Mr. Evans after Dr. Boyd was removed does not indicate that Dr. Boyd's removal was not political. Dr. Boyd having been removed, Mr. Evans may have been, for aught we know, the best candidate, or the only candidate, in sight. The one essential fact that appears in this whole miserable business is that the president and a large proportion of the faculty have been summarily removed from office, and that there is no pretense that any question of their scholarly attainments or their competence to teach was involved in the removal. To the charge that the reasons for the removal were political, ecclesiastical and personal favoritism, only one answer is possible. That answer is a clear statement of some other reason; and no other reason is even suggested. —*The Outlook.*

SCIENTIFIC BOOKS

Introduction to Metallography. By PAUL GOERENS, Docent in Physical Metallurgy at the Royal Technical High School, Aachen. Translated by FRED IBBOTSON, Lecturer in Metallurgy, The University, Sheffield. London, Longmans, Green and Co. 1908. Pp. x + 214.

The applications of physical chemistry to the solution of industrial problems have been many, but it is doubtful whether any field has yielded such important results as in the study of metallic alloys. Empirically established facts have been placed upon a distinctly scientific basis and fortuitous experimentation has been replaced to a very great extent by accurate prediction. Perhaps in no other field of chemistry or physics has there been such an accumulation of unsystematized observations. This information is now being carefully classified by the results of metallographic study. The methods used are thermal and microscopic—the study of freezing-point curves and the microscopic examination of alloys of varying concentration. The author of this treatise has rendered a distinct service to those interested in the study of the properties of metals. The information has been widely distributed and unavailable to many, and it is now brought together in compact form. The book is simply and clearly written and is an excellent guide to the study of metallography. The exposition of the theoretical side of the subject is not as complete as it might have been but it will give the beginner an excellent idea of equilibrium phenomena. The explanations of the freezing-point diagrams have been duplicated unnecessarily, perhaps not for the beginner, but certainly for those using the book for reference. For the latter class of readers there is too much detail.

The volume treats of the methods employed in the establishments of cooling curves, the interpretations of these curves with chapters on the analogies between aqueous solutions, fused salts and alloys; the practical microscopy and photography of metallic sections; and a special chapter on the iron-carbon system. In the translation this chapter has been completely rewritten.

The author has, in a number of cases, without any apparent reason given the older freezing-point diagrams of Gautier, and Roland-Gosselin along with the more correctly established diagrams of later workers. This arrangement occupies space at the expense of clearness. The placing of the diagram on the page might also have been done to better advantage. As an example, on page 110 the copper-nickel diagram is given under the paragraph heading silver-zinc, and the diagram for this pair of metals is given on the following page under copper-tin.

In the explanation of those diagrams in which a concealed maximum exists, the changes in concentration are not clearly followed and in at least one case inaccurately given.

The chapter on iron-carbon alloys is clear, concise and well illustrated with excellent reproductions of photomicrographs.

Henry Fay

SCIENTIFIC JOURNALS AND ARTICLES

The contents of *Terrestrial Magnetism and Atmospheric Electricity* for September are: "Pocket Compass Sun-Dial of 1451" (frontispiece); "The Earliest Values of the Magnetic Declination," by L. A. Bauer; "On the Distribution of Magnetism over the Earth's Surface," by P. T. Passalskij, translated by Paul Wernicke; "Report on the Atmospheric Electricity Observations made on the Magnetic Survey Yacht, *Galilee,* 1907–8," by P. H. Dike; Letters to Editor; Notes; Abstracts and Reviews; List of Recent Publications.

THE RULE OF PRIORITY IN ZOOLOGICAL NOMENCLATURE[1]

Disapproval was expressed of the extreme application of the rule of priority, which in the author's opinion had brought about much mischief under pretence of aiming at ultimate uniformity. The author protested against the abuse to which this otherwise excellent rule had been put by some recent workers, encouraged as they were by the decision of several committees who had undertaken to revise the Stricklandian Code, elaborated under the auspices of the British Association in 1842. The worst feature of this abuse is not so much the bestowal of unknown names on well-known creatures as the transfer of names from one to another, as we have seen in the case of *Astacus, Torpedo, Holothuria, Simia, Cynocephalus,* and many others which must be present to the mind of every systematist.

The names that were used uniformly by Cuvier, Johannes Müller, Owen, Agassiz, Darwin, Huxley, Gegenbaur, would no longer convey any meaning, very often they would be misunderstood; in fact the very object for which Latin or Latinized names were introduced would be defeated. It is all very well to talk of uniformity in the future, but surely we must have some consideration for the past. Names with which all general zoologists anatomists and physiologists are familiar should be respected, should be excepted from the rule in virtue of what may be termed the privilege prescription.

If biologists would agree to make that one exception to the law of priority in nomenclature things would adjust themselves well enough, and we might hope to see realized some day what we all desire, fixity in names, that we may readily understand the meaning of all writers, not only over the whole civilized world, at the present day and in the future, but back into the last century, which has marked so great an advance in zoological science. Such a result would be attained by protecting time-honored names of well-known animals from the attacks of the revisers of nomenclature. For this purpose future committees that may be convened to discuss these topics might confer a real and lasting benefit on zoology by determining group by group, which names are entitled to respect, not, of course, on the ground of their earliest date or their correct application in the past, but as having been universally used in a definite sense.

This suggestion is not a new one. As far back as 1896, in a discussion which took place at the Zoological Society of London, Sir Ray

[1] Abstract of a paper by G. A. Boulenger, F.R.S., presented at the Dublin meeting of the British Association.

Lankester, protesting against the digging up of old names, suggested that an international committee should be formed, not to draw up a code of rules, but "to produce an authoritative list of names—once and for all—about which no lawyer-like haggling should hereafter be permitted. Twelve years have elapsed, and nothing of the kind has been arranged. On the contrary; the various committees that have legislated since have insisted on absolute priority, and we often read that such a decision has been arrived at by international agreement. It is not so, a great body of zoologists in this country protest, and hope that something will be done towards carrying out the proposal here briefly set forth, which seems to be the only proper step to take in order to prevent the confusion with which we are menaced.

SPECIAL ARTICLES

SOME RESULTS OF A SERIES OF TESTS MADE BY THE WIRE-BASKET METHOD FOR DETERMINING THE MANURIAL REQUIREMENTS OF SOILS

A NUMBER of methods for determining the manurial requirements of any given piece of land have been proposed from time to time, since it is well known that the mere chemical analysis of a soil often fails to be of real value in this connection.

One of the most reliable methods consists in making actual field tests with various fertilizers applied in definite amounts to plots of land of equal size, one or more of the plots being left untreated to serve as a check. The effect of the various manures applied is measured in terms of the crop harvested, and thus the requirements of the soil for specific forms of plant food is made evident. The chief objection to this method is that climatic conditions are not always favorable for the best results in any one season, in addition plant diseases and insect pests may be active, hence it often happens that it becomes necessary to conduct the field experiments for a number of years before definite conclusions can be reached.

During the year 1904, the Bureau of Soils of the United States Department of Agriculture, devised a promising method for ascertaining the manurial requirements of soils. This has been published as Circular 18, Bureau of Soils, "The Wire-Basket Method for Determining the Manurial Requirements of Soils." Briefly, it consists in treating samples of the soils in question with definite amounts of various fertilizers, and placing the treated soil samples in wire-baskets which are then coated with melted paraffin, and growing wheat in the soil in these baskets for short periods of time. The amount of water transpired by and the green weight of the plants are taken as indicative of the requirements of the soil for specific manurial constituents. The value of the method consists largely in the fact that results can be obtained in a period of two or three weeks.

In the early part of 1908 the writer was requested to make a series of wire-basket tests of soil samples from Boydton, Va.; these samples were taken on a farm, the property of the Boydton Institute. The soil of this region is characterized by one who has worked it for a number of years, "as having tilling qualities of about the average for a heavy clay soil . . . if plowed at the proper time, subsoiled and kept stirred it presents no unusual difficulties." The two soils represented by the following samples are undoubtedly of the same character, and differ from each other mainly through the different treatment which each has received.

A rough mechanical analysis of one of the samples (*a*) by the beaker method, made in this laboratory, gave the following result: Sand 29 per cent., clay 18 per cent., silt 53 per cent. A deficiency of humus was shown by the small amount of volatile matter present (4.76 per cent.) and by the absence of a dark color in the soil. By actual determination the amount of humus was found to be 1.40 per cent. The gravel was found to be composed mainly of quartz.

Two samples of soil were used in making these tests, designated as (a) and (j), having the following history: (*a*) "East end of corn lot on the 300-acre tract. Cleared about

1887. Cropped with corn and oats (poorly) every second or third year and idle between. Fertilized 1907 with a cheap corn fertilizer. Yield 10 to 12 bushels of corn per acre."

(j) "Cherry tree lot, west of campus. Probably cropped since 1894. Was once part of the campus. Cropped with corn and potatoes, alternately, clover and oats also planted twice, potatoes in 1906, millet in 1907, but failed because of drought. Some sweet potatoes planted there in 1907 but were also poor because of the dry season. Corn yielded there about 40 bushels to the acre. None of this land has been fertilized more than twice in thirty years and then with wood ashes or a cheap commercial fertilizer."

These soil samples were subjected to the following treatment: The air-dry soil was rolled with a small wooden roller to crush the lumps and sifted through a sieve having meshes of 2 mm. Three-pound portions of the pulverized soil were each placed in a separate tray moistened with distilled water and treated with various fertilizers.

In the case of sample (a) to portion I. was added nitrogen, as nitrate of soda, at the rate of 200 lbs. to the acre; and phosphorus, as acid phosphate (containing 14 per cent. of soluble phosphoric acid) at the rate of 600 lbs. to the acre. Portion number II. received nitrogen, as sodium nitrate, at the rate of 200 lbs. per acre, and potassium, as sulphate of potash at the rate of 200 lbs. to the acre. Number III. received phosphorus, as acid phosphate (14 per cent. soluble phosphoric acid), at the rate of 600 lbs. to the acre; and potassium, as sulphate of potash, at the rate of 200 lbs. to the acre. Number IV. received nitrogen, as nitrate of soda, 200 lbs. to the acre; phosphorus, as acid phosphate (14 per cent.), 600 lbs. per acre; and potassium, as sulphate of potash, 200 lbs. per acre. Number V. received the same treatment as number IV., and in addition calcium, in the form of slaked lime, at the rate of 2,000 lbs. per acre. Number VII. received nothing and served as a check.

Soil sample (j) was divided into five three-pound, portions. Numbers I., II., III. and IV. received precisely the same treatment as did numbers I., II., III. and IV. of sample (a). Number V. served as a check and was left untreated.

All of these various portions were allowed to remain in their respective dishes for several days, with occasional stirring and moistening in order to thoroughly distribute the fertilizers. Then, as nearly as could be judged, enough distilled water was added to each portion to supply the optimum or most favorable moisture content for plant growth, which according to Hilgard, is equivalent to from 40 per cent. to 60 per cent. of the water capacity of a soil. Each portion was divided into three approximately equal parts, and each part was placed in a wire basket; these baskets were of standard dimensions, 3 inches in diameter, 3 inches deep, mesh 3 mm., and contained about 1 pound of the prepared soil, which was well pressed into the bottom and sides of the baskets. The baskets containing the soil were then repeatedly dipped into a bath of melted paraffin-wax until a good coating was obtained. Six germinated kernels of wheat were planted in a row in the soil of each basket and a thin layer of clean white sand was placed on the top of the soil. The filled baskets were then weighed and placed in a greenhouse where favorable conditions for growth were maintained.

When the wheat plants had reached a height of about two inches enough distilled water was added to each basket to bring the weight up to the first weighing, and a paraffined paper disk was fitted about the stems of the plants and sealed to the sides of the basket by means of a small amount of melted paraffin-wax. By this device all escape of moisture from the soil was practically cut off except by way of transpiration through the plants. The baskets were again weighed, and thus the optimum weight was ascertained. The baskets were then returned to the greenhouses and maintained under favorable conditions for growth.

The amount of water transpired by the plants was ascertained by weighing each basket at intervals of three or four days.

After each weighing enough distilled water was added to bring the weight slightly above the optimum.

The experiment with sample (a) was carried on for fourteen days after the baskets were sealed. With the sample (j) the test was conducted for eighteen days. At the end of these periods the green plants were cut off close to the paper disks and weighed.

The following table gives a summary of the results of these tests.

SOIL SAMPLE (a)

Series Three Baskets Each	Fertilizing Constituents Added	Total Water Transpired. Grams.	Weight of Green Plants. Grams.
I.	Nitrogen and phosphorus	134.9	2.796
II.	Nitrogen and potassium	165.2	3.840
III.	Phosphorus and potassium	102.0	2.130
IV.	Nitrogen, phosphorus and potassium	129.6	3.083
V.	N, P, K and calcium	144.4	3.445
VI.	Calcium	102.3	1.850
VII.	Blank, nothing	112.3	1.880

In this table a comparison of series I. with IV. and of I. with II. indicates a lack of potassium in the soil. A comparison of II. with IV. and of II. with III. shows but little, if any, deficiency of phosphorus in the soil. A comparison of III. and IV. and of III. with II. clearly shows a deficiency of nitrogen in the soil. With regard to calcium in the form of lime a comparison of VI. and VII. indicates that lime alone is of little or no value. It is of interest to note here that the actual application of lime alone to this soil under field conditions did not give any increase of crop. But, as is well known, the application of lime alone will give poor results on land deficient in nitrogen and in mineral constituents, especially potassium compounds. A comparison of V. and IV. indicates that lime used in conjunction with mineral fertilizers may be of value on this soil. In this table, as well as in the following one, it is noteworthy that the amount of water transpired keeps pace with the weight of the green plants; that is, the larger the green weight of the plants the greater is the total transpiration.

SOIL SAMPLE (j)

Series Three Baskets Each	Fertilizing Constituents Added	Total Water Transpired. Grams.	Weight of Green Plants. Grams.
I.	Nitrogen and phosphorus	160.5	2.970
II.	Nitrogen and potassium	212.5	4.278
III.	Phosphorus and potassium	146.7	2.632
IV.	Nitrogen, phosphorus and potassium	203.2	3.841
V.	Blank, no fertilizer	196.8	3.295

In this table a comparison of series I. with IV. and of I. with II. shows a deficiency of the soil in potassium. A comparison of II. with IV. and of II. with III. does not indicate a lack of phosphorus; in fact, it raises the question as to whether or not the acid phosphate may have done actual harm in the case of III. With regard to nitrogen a comparison of III. with IV., with II. and with I in turn shows a deficiency of this element.

On the whole the results of tests with samples (j) agree with the results of those with sample (a). Though from these tests sample (j) would seem to be in a better chemical condition than sample (a), in that (j) responds less readily to treatment with fertilizers than does (a). Compare VII. of Table (a) with the other series of Table (a) and V. of Table (j) with the other series of Table (j). A bit of evidence bearing out this contention is to be found in the statement that soil (j) has actually yielded 40 bushels of corn per acre while soil (a) has given but 10 to 12 bushels of corn to the acre.

From all these tests it appears that the soil of the Boydton Institute is deficient mainly in nitrogen and potassium and in humus. As a result of these tests and in view of the difficulty of obtaining a sufficient supply of farmyard manure at Boydton, the writer has advised the use of green manuring, for this purpose turning under cow peas, or crimson clover, to be stimulated by the application of potassium, in the form of muriate of potash, and small dressings of available phosphates.

The president of the Boydton Institute, Mr. John R. Hague, at whose instigation these experiments were made, has agreed to conduct

at Boydton field tests which shall be comparable to these wire-basket experiments. The main crops to be grown are corn and wheat.

Frank T. Dillingham

Bussey Institution,
Harvard University,
April, 1908

Sudan III. deposited in the egg and transmitted to the chick

In repeating the experiments reported by Dr. Oscar Riddle[1] in Science, June 19, 1908, p. 945, for the purpose of demonstration before the third session of the Graduate School of Agriculture of the United States, held July 6–30, 1908, at Cornell University, the results obtained by Dr. Riddle were confirmed; *i. e.*, Sudan III. fed to hens during the laying period stained red the layers of yolk deposited during the feeding of the Sudan. The amount of the stain used was much greater (20 to 25 milligrams at a dose) than Dr. Riddle for his special purpose found desirable, hence the yolks were strikingly colored, and always in concentric layers. Even when all the food eaten by the hen during the entire twenty-four hours was mixed with Sudan, the layers of the yolk were marked.[2]

[1] Riddle's paper was presented at the zoological meetings during convocation week at Chicago, 1907–8. He kindly gave to the authors personally the technique necessary for obtaining the colored eggs.

[2] If any of the readers of Science desire to experiment with Sudan III. the following hints may be of service: Sudan III. may be purchased of any dealer in microscopic supplies, *e. g.*, the Bausch and Lomb Optical Company, Rochester, N. Y. It is practically tasteless and the dry powder may be mixed with the food or it may be dissolved in olive oil and that mixed with the food. The dose is small (for a hen, 3 to 25 milligrams). The larger doses give more brilliant coloration. Water, glycerin and formalin do not dissolve either fat or Sudan III., hence watery solutions of glycerin and formalin are good preservatives of tissues containing the stained fat. The most satisfactory preservative found both for the eggs and for the entire animals containing colored fat is 5 per cent. formaldehyde. (Strong formalin 10 parts, water 70 parts.) Eggs are best prepared by boiling 15 to 20 minutes, then the shell is removed from the large end and the white and yolk cut off in thin slices, holding the egg and knife under water. Sections through the germinal disk are the most instructive. For permanent support and preservation of the prepared eggs glycerin jelly has been found satisfactory.

In order to carry the investigation a step farther and to answer the question whether this coloring matter could be carried over to the chick, some of the "red eggs" were incubated, with following results:

1. As the yolk softened during the processes of growth of the embryo the layered mass became homogeneous and of a uniform pink. This was marked from the third day onward. For the first ten days the transparent embryo showed no sign of the color.

2. As soon as the developing chick began to deposit fat, at the seventeenth day of incubation, a minute mass of fat lying in the loose connective tissue between the leg and the abdomen was found with the characteristic pink color which depositing fat takes in adults fed with this stain. At this time the egg mass is of a nearly uniform dark red and almost enclosed within the body.

3. At hatching and several days thereafter the pink fat increased in amount, extending along the side of the sternum, the neck and head and finally appearing on either side of the back in the pelvic region. At the same time the yolk was losing its red color.

4. It was hoped that the peculiar fat of nerve fibers might take up some of this stain in the period during which myelinization is rapidly proceeding; but the nerve tracts showed only their usual glistening white.

To briefly summarize:

1. The specific fat stain, Sudan III., colors the fat laid down in the living hen and in the fatty portions of the yolk while the feeding experiments are in progress, and thus serves to give exact data concerning the time and amount of deposit.

2. The eggs so colored hatch, and the chick utilizing the yolk as food, produces fat in its own body colored as in the adult, showing in the most striking manner the transmission of a specific and unusual or foreign substance

from the mother to the egg, and from the egg to the offspring, and thereby marking the transmission of the actual substance of the egg, and indirectly of the mother, to the offspring.

3. The precision of the method and its striking results apparently open to biologists a field which has lain dormant since its discovery by Daddi[3] twelve years ago, *i. e.*, the possibility of following with great exactness at least one of the processes of nutrition.[4]

SIMON H. GAGE
SUSANNA PHELPS GAGE

GEOLOGY AT THE BRITISH ASSOCIATION

THE seventy-eighth annual meeting of the British Association for the Advancement of Science was held at Dublin, September 2 to 9, largely in the buildings of Trinity College and the Royal University. The Lord Lieutenant of Ireland, the universities, and the city cooperated to extend to the visitors a true Irish hospitality. In addition to the presidential address by Dr. Francis Darwin, popular illustrated evening addresses were delivered by Professor Turner, of Oxford University, on "Halley's Comet," and by Professor Davis, of Harvard University, on "The Lessons of the Colorado Canyon."

The mornings only were devoted to the reading of papers, the afternoon hours being wholly taken up by excursions and by elaborate social functions. In the sectional meetings an innovation was introduced in a large bulletin board set up at the front of each room, on which appeared "Papers Now Being Read." Beneath this were placed large letters to designate the individual sections of the association, and under each was a peg on which was hung up the number of the paper which at the moment was being read. A boy in attendance almost noiselessly received the telephonic messages at one end of the presiding officer's desk and adjusted the numbers on the board after the manner in use at American football games. The method proved a success, and might well be adopted by the American Association. As some of the section rooms were rather widely separated, an inter-section service of automobiles running at ten-minute intervals was instituted, but of its success the writer is unable to speak.

The invitation of the city of Winnipeg for the association to hold its meetings of 1909 in that city was brought by Dr. Bryce, vice-president of the Royal Society of Canada, and was accepted. The meetings will be held at Winnipeg during the last week of August and will be followed by an excursion through the Canadian Rockies by special train to Vancouver, B. C., with stops at Banff, Glacier and other intermediate points. To members of the British and American Associations the trip will be made for one fare, or $50, and an excursion from Vancouver to Alaska and perhaps to still more distant points is under consideration. To members of the American Association attending the meetings the usual dues of a sovereign will be remitted, and it is hoped that the occasion will be notable by reason of the large number of British, Canadian and American scientists brought together. As was the case on the occasion of the South African meeting of the British Association, a considerable number of distinguished scientists will be made the special guests of the occasion.

Section C (Geology) was well attended by representative geologists of Great Britain, and the distant dependencies of the empire were represented by Hume, of Egypt; Hayden, of India; Maitland, of Western Australia; Hatch, of South Africa, and Grabham, of the Soudan.

The address before the section was delivered by Professor John Joly, F.R.S., of Trinity College, Dublin, the president of the section, on "Uranium and Geology." It was an able and scholarly address dealing with the recent developments in the study of radio-activity as a factor in geological dynamics, and ascribing to it large importance in the explanation of the earth's interior heat and of mountain growth. Professor Joly's own studies in connection with the great Alpine tunnels, where he found the least quantity of radium corresponded to the greatest depth below the surface, he explained by the radio-active nuclei originally distributed fortuitously through the earth's mass, heating and expanding beyond the capacity of the surrounding material, and in consequence rising to the surface. It would thus seem that it is not necessary to assume such large quantities of radium to be still contained in the core of the earth as are now to be found in its outer shell.

[3] Daddi, in 1896 (*Arch. Ital. de Biol.*, T. XXVI., 1896, p. 143), was the first to show that fat deposited in the living body is stained by Sudan III. when ingested with the food.

[4] The authors wish to express their thanks to the staff of the poultry department of the New York State College of Agriculture for the abundant facilities and assistance placed at their disposal.

Dr. Milne, the veteran earthquake specialist, read a paper on "The Duration and Direction of Large Earthquakes," in which he called attention to an apparent tendency of large earthquake waves to travel farther to the west than to the east, and to an apparent difficulty which they encounter in getting across the equator. Professor W. M. Davis discussed "Glacial Erosion in North Wales," laying stress especially upon the prevalent corrie, the broad valley, and the hanging side valley, as undeniable evidences of the important eroding power of glaciers.

Dr. Tempest Anderson made a report upon "Changes in Soufrière of St. Vincent" based upon a recent trip to the island and upon his earlier visit made just after the great eruption of 1902. Of special interest are the changes observed in the torrent of hot mud which had filled the Wallibu Valley to a height in places of more than a hundred feet. The greater part of this deposit has since been washed away, fragments remaining, however, in a terrace 60 to 80 feet high on the north side of the valley. In general it may be said that in place of each of the earlier valleys there are now found two parallel valleys developed on either side of the mud streams which have filled them. Each present valley thus has for one of its walls the original valley wall and for the other a cliff cut in the ash of 1902.

Professor Lapworth contributed a report upon important revelations made by excavation through critical sections in Shropshire and North Wales. A very interesting case of thrust and crush brecciation in the Magnesian Limestone of County Durham was presented by Dr. Woolacot.

The most interesting feature of the meeting to the geologists was a formal symposium on the subject of mountain building, to which Professor Joly, Sir Archibald Geikie, Professor Lapworth, Professor Sollas and Professor Cole contributed. Professor Joly in opening the discussion focused attention upon the Alpine type of mountain, and credited largely to Professor Lugeon the discovery of the great overturned folds and thrusts of the northern Alps. Radio-activity was brought into the problem so as to connect the areas of mountain elevation with areas of sedimentation. The immediate surface rocks were of such richness in radium as to preclude the idea that a similar richness would extend many miles inward. Now as the sediments grew in thickness this original layer was depressed deeper and deeper, yielding to the load until at length it was buried to the full depth of the overlying deposit. Here the law of increase of temperature with the square of the depth came in, and the effect of the accumulated sediment was thus a reduction in the thickness of that part of the upper crust capable of resisting a compressive stress. Along this elongated area of weakness the crust found relief and was flexed upwards.

In continuing the discussion Professors Geikie and Lapworth took the ground that the revelation of Alpine structure must be largely credited to earlier geologists, and especially to Heim and Bertrand, whose observations and conclusions had been elaborated and very ably presented by Professor Lugeon. By Professor Lapworth mountains were discussed especially in relation to the plan of the earth, the distribution of the great mountain arcs, and the relation of the elevation of shores to the depression of the neighboring sea-bottom, in connection with which treatment he paid a glowing tribute to Eduard Suess.

Professor Sollas described a recent excursion which he had made under the leadership of Professor Lugeon in examination of the great overthrusts exhibited about the lake of Lucerne. The frequent location of active volcanoes at the rear of growing mountain ranges—on the side from which the overthrust was exerted—was explained by the use of Willis's law of competency of structure. The elevation of a competent layer of rock relieved the pressure from underlying rock material, thus allowing it to fuse and shift laterally in the direction of the rear of the fold.

The numerous excursions of the geologists to points in the vicinity of Dublin were most instructive and offered greatest interest to students of structural geology. The excursion of Saturday to the Skerries under the leadership of Dr. Matthies afforded the opportunity of examining a section which revealed in great perfection all the common types of folds, including small recumbent folds and overthrusts. The relation of gashing and healing to the position of arches and limbs was strikingly brought out. Professor Cole and Mr. Seymour conducted another all-day excursion to the Devil's Glen and Glendalough through some of the most picturesque sections of County Wicklow. Here the contact zones of great batholiths of granite in surrounding shistose rocks were well displayed. Other interesting excursions were conducted by Professor Reynolds and Mr. Muff.

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